

# Fundamental Disputations: The Philosophical Debates that Governed American Physics, 1939–1993

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**ABSTRACT:** A philosophical debate between particle physicists and solid state physicists roiled as these sub-disciplines competed for financial support, social approbation, and intellectual prestige through the second half of the twentieth century. Their disagreement hinged on the nature of fundamental research. The particle physics community adopted a reductionist approach, arguing that the fundamental physical laws were those governing the smallest constituents of matter and energy. In part in response to this position, solid state physicists developed a range of more permissive perspectives on what type of physics could be

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The following abbreviations are used: APS, American Physical Society; CRS, Correspondence of Roman Smoluchowski, American Institute of Physics, Niels Bohr Library and Archives, College Park, MD; FBP, Francis Bitter Papers, Massachusetts Institute of Technology Archives, Cambridge, MA; *HSPS*, *Historical Studies in the Physical and Biological Sciences*; MIT, Massachusetts Institute of Technology; NAL, National Accelerator Laboratory; NBL, Niels Bohr Library and Archives, American Institute of Physics, College Park, MD; NML, National Magnet Laboratory; NMLR, Francis Bitter National Magnet Laboratory Records, 1957–1982, Massachusetts Institute of Technology Archives, Cambridge, MA; NSF, National Science Foundation; *PT*, *Physics Today*; SSC, Superconducting Super Collider.

fundamental, all of which stressed the importance of higher-level characteristics, maintaining that investigations at many levels of complexity might yield fundamental insight. This paper traces the dispute over fundamentality, which grew both from the specific problems physicists encountered while building their professional infrastructure, and from the demands of funding their research in Cold War America. Through an exploration of how physicists developed philosophical positions within institutional contexts and deployed those positions in their rhetoric, I argue first that professional pressures both motivated and exerted influence over the construction of such views, second that philosophical views had a reciprocal guiding effect on the institutional and professional development of Cold War physics, and third that these views were further bent, blunted, and reshaped when deployed in high-stakes rhetorical discourse. The case studies through which this story unfolds indicate that further attention to such philosophical commitments is warranted when examining the historical development of scientific institutions, communities, and hierarchies.

KEY WORDS: Solid State Physics, Fundamentality, Reduction, National Magnet Laboratory, Superconducting Super Collider, Francis Bitter, Philip Anderson, Steven Weinberg

ABBREVIATED TITLE: Fundamental Disputations

## [FIRST LEVEL HEADING] INTRODUCTION

What does it mean for physics to be fundamental? That question pervaded physicists' efforts to organize their rapidly expanding discipline through the second half of the twentieth century. Solid state physics and particle physics, respectively the most populous and most

prestigious branches of Cold War American physics, negotiated their relationship through an intricate and delicate dance as they grew into distinct sub-disciplines. They often found themselves competing for funding, institutional resources, personnel, and public approbation. At other times, they allied to advocate for the importance of physics as a whole to American society. Within these negotiations, which ranged from cautious diplomacy to blood sport, the authority to define the term “fundamental” as applied to physics was hotly contested turf. This paper traces a protracted philosophical debate over the nature of fundamentality. It demonstrates how physicists’ philosophical attitudes towards fundamentality, themselves motivated and defined by professional and institutional challenges at several scales, in turn guided subsequent institutional and professional developments in American physics.<sup>1</sup>

Any discussion of fundamentality naturally suggests questions of reduction and emergence, which have garnered considerable attention from both historians and philosophers.<sup>2</sup> It was a critical element of fundamentality debates, but also at issue were questions about how close of a relationship physics should maintain with applications, how physics should connect

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<sup>1</sup> These debates did not neatly group solid state physicists on one side and particle physicists on the other; however, these categories do describe the core interlocutors. The relevance of many of the stances described here to the institutional and professional goals of each sub-disciplinary group makes the otherwise overly broad dichotomy an instructive one.

<sup>2</sup> See, for example: Helge Kragh, *Higher Speculations: Grand Theories and Failed Revolutions in Physics and Cosmology* (Oxford: Oxford University Press, 2011); Mark A. Bedau and Paul Humphries, eds., *Emergence: Contemporary Readings in Philosophy and Science* (Cambridge, MA: MIT Press, 2008); Robert W. Batterman, *The Devil in the Details: Asymptotic Reasoning in Explanation, Reduction, and Emergence* (Oxford: Oxford University Press, 2002).

with other scientific disciplines, and how it should be internally ordered. Although the reduction-emergence dichotomy is one way in which the debate broke down, appreciating the range of influence fundamentality debates had on American physics will require considering these other factors alongside it.

With that in mind, I propose understanding physicists' views on fundamentality on two axes: first according to whether they are restrictive or permissive, and second according to whether they define fundamentality as a relational or as an innate property of scientific knowledge. Restrictive positions propose that fundamental insight occurs only at one scale, whereas permissive positions maintain that such insight can be had at many different scales. Relational fundamentality proposes that scientific knowledge becomes fundamental by virtue of how it connects with other knowledge, scientific or otherwise, whereas the innate view suggests that scientific knowledge is fundamental by virtue of the type of knowledge it is.

Of the four possible combinations defined by these two axes, three were at play during the era under investigation. The restrictive/innate view took the form of a reductionist position articulated by particle physicists, who argued that fundamental knowledge was found exclusively among the smallest components of matter and energy.<sup>3</sup> Solid state physicists and their allies explored both types of permissive views at various times. The first, the permissive/relational view, is defined by a quality I call fecundity. The degree to which physical knowledge found new and novel uses in other research areas was one common measure of how fundamental it is.

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<sup>3</sup> Though this perspective jibes well with reductionism, it is not necessarily reductionist—a restrictive perspective might deny physical hierarchy entirely, for instance. See: Miriam Thalos, *Without Hierarchy: The Scale Freedom of the Universe* (Oxford: Oxford University Press, 2013).

Within this category, individuals emphasized different types of cross-field relevance. For some, the discovery of general principles with a wide range of applicability was the relevant measure. Others looked to technological, economic, and social relevance in addition to scientific relevance. The permissive/innate view manifested as emergence: fundamentality defined by the independence of laws or concepts at multiple scales of complexity. These laws and concepts can be independent either in practice or in principle, and we can find examples of both arguments in the rhetoric of twentieth-century physics.

The most relevant positions articulated by working physicists in mid-to-late twentieth century America were thus defined predominantly by reduction, fecundity, and emergence. These categories are not those of the actors, in part because the debate described here was a complex and messy one, in which interlocutors often talked past one another and neglected to define their terms to the level of precision that would satisfy a professional philosopher. I do not intend to reify this scheme, but to loosely categorize the confoundingly diverse array of positions physicist took on the fundamentality question over the half-century or so covered by the case studies in this paper. These examples describe how American physicists' stances towards fundamentality evolved. Each case considers the distinctive characteristics the relevant views assumed in different contexts and at a different level of applicability. At each of these levels of scientific organization—individual research installations, prestige hierarchies within physics, and the national funding context—physicists' convictions manifested themselves differently, and with different results.<sup>4</sup>

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<sup>4</sup> John A. Schuster and Richard A. Yeo, "Introduction," in *The Politics and Rhetoric of Scientific Method*, ed. John A. Schuster and Richard A. Yeo (Dordrecht: D. Reidel, 1986), ix–xxxvii, esp. xi–xv, set out a similar hierarchy. They claim that the rhetoric of scientific method operates on

I begin with the National Magnet Laboratory (NML) at the Massachusetts Institute of Technology (MIT). The NML began operations in the early 1960s, realizing goals its founder, Francis Bitter, had pursued since the late 1930s. Bitter viewed fecundity as the primary component of fundamentality and his convictions shaped the lab's research objectives and pedagogical priorities.<sup>5</sup> The NML's autonomy to pursue its mission, however, was challenged by the funding cutbacks just a few years after it opened. As the only large national facility dedicated to high magnetic fields at the time, its direction became determined more by how it fit within a

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three levels of discourse. First, in the laboratory, it provides a way to rationalize an often messy and non-linear process of knowledge production. Second, they identify the institutional organization of the sciences as the level on which scientists negotiate disciplinary politics. Finally, they discuss how those who articulate science in wider social and cultural contexts deploy the rhetoric of the scientific method. I identify similar hierarchical divisions, but contend that their boundaries are fuzzy and their interactions rich, and argue that the views developed and used within each level functioned as more than mere rhetorical strategies.

<sup>5</sup> The term "philosophical" is slippery in this context. Physicists are not professional philosophers and it would be a stretch to call the physicists' convictions about best practices "philosophies." Physicists did, however, articulate bona fide, self-consciously philosophical stances in response to changing contextual factors in the 1960s and 1970s. Because these views articulated foundational principles to guide physicists' understanding of physical knowledge, and to place pre-existing beliefs about intellectual prestige, scientific merit, and rational resource allocation on a well-considered foundation, they are philosophical in a substantive sense. Understanding their precursors, whether or not they achieved the same degrees of rigor, is therefore essential to assess their historical importance.

nationwide ecology of large laboratories and how far it could bend to meet prevailing funding priorities. This case study demonstrates both the potential for views such as Bitter's to shape individual labs, and their limitations in the face of large-scale national trends.

The second case study interprets explicit expressions of philosophical positions by working physicists and shows how changes within the physics community during the late 1960s and early 1970s motivated physicists to develop and articulate otherwise tacit philosophical views. Anderson, in the famous 1972 article "More Is Different" was motivated to articulate a detailed defense of emergentism in part by the growth of reductionism in the particle physics community, and the consequences for the professional landscape of Cold War physics. The same funding pressures and disciplinary tensions that threatened the National Magnet Laboratory's mission prompted Anderson and others to revisit the types of convictions that had grounded Bitter's vision for his laboratory and to refine them into philosophical platforms that could underwrite claims for intellectual recognition, disciplinary authority, and federal funding. In this way, changes in the professional politics of physics motivated individuals to examine reinforce the philosophical foundations of their research.

Finally, disagreements over the merits of the Superconducting Super Collider (SSC) in the late 1980s and early 1990s indicate how those views fed into debates about national science policy. Having developed strong philosophical justifications for their work through the preceding decades, solid state and particle physicists were prepared to press them into service both in Congress and in the court of public opinion. As they locked horns over the direction of science funding in the United States, their philosophical squabble became concrete, and so again their views adapted to meet the needs of the moment. Anderson, for instance, backed off the innateness of fundamentality and embraced relational arguments he had avoided in the 1970s.

Similarly, particle physicists, while still espousing reductionism, scrambled to show that their intellectual output had meaningful relationships both with technology and with other areas of science. The case of the SSC, like that of the NML, shows us both the potential and the limitations of scientists' philosophical commitments. Those commitments provided a framework in which debates over institution building, prestige, and federal funding played out during the SSC controversy. But they also became blunted by use. Philosophical convictions, however devoutly held, proved little match for the existing expectations of federal funders. In becoming shibboleths for disciplinary communities, physicists' philosophies intertwined with realities of practice and bent to meet the political exigencies of the moment. That physicists' philosophical convictions proved malleable against the force of national funding priorities, however, does nothing to diminish the subtle but pervasive influence they exerted over the development of Cold War physics at several levels

Elements of these case studies have been examined in the literature through the lens of unity.<sup>6</sup> Jordi Cat, in his discussion of Anderson's arguments for emergence, observes that solid state physicists "place[d] the burden of unity in methodology," while their particle physicist counterparts advocated reductive unity.<sup>7</sup> In a similar vein, Hallam Stevens describes how

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<sup>6</sup> As in Peter Galison and David J. Stump, eds., *The Disunity of Science: Boundaries, Contexts, and Power* (Stanford: Stanford University Press, 1996).

<sup>7</sup> Jordi Cat, "The Physicists' Debates on Unification in Physics at the End of the 20th Century," *HSPS* 28, no. 2 (1998): 253–299, on 254. The question of whether or not a piece of physics was fundamental is meaningfully different from the question of how physics is unified, especially when examining professional contexts. Unity was central when physicists reflected upon their field as a whole, particularly with respect to other sciences. When internecine tensions



reductionism grew within the particle physics community in part because the theoretical power of the symmetry concept underwrote particle physicists' claims to ultimate knowledge, which in turn allowed them to distance themselves from military research when it became controversial in the mid-1960s.<sup>8</sup> Cat, Stevens, and others have tended to consider conceptual factors when examining the relationship between philosophy and physics.<sup>9</sup> Richard Staley has recently observed this trend and commented that “historians have done too little to illuminate the broader disciplinary context” philosophical debates between physicists reflect.<sup>10</sup> Physicist Max Dresden, although he was not addressing historians, has also argued for the centrality of philosophical conviction to professional and institutional development. By way of attacking the absolutism associated with some brands of reductionism, Dresden insisted that “[j]ust what is surprising, unexpected, new, or important depends on the total intellectual, emotional status of the individual and his or her personal reactions to the philosophical, social, political environment, as

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developed, however, physicists turned to fundamentality as a way to rank and privilege competing subfields.

<sup>8</sup> Hallam Stevens, “Fundamental Physics and its Justifications, 1945–1993,” *HSPS* 34, no. 1 (2003): 151–197.

<sup>9</sup> The philosophical literature on effective field theories is another notable example. See: Jonathan Bain, “Effective Field Theories,” in *The Oxford Handbook of Philosophy of Physics*, ed. Robert W. Batterman (Oxford: Oxford University Press, 2013), 224–254 and Elena Castellani, “Reduction, Emergence, and Effective Field Theories,” *Studies in History and Philosophy of Modern Physics* 33, no. 2 (2002): 251–267.

<sup>10</sup> Richard Staley, “Trajectories in the History and Historiography of Physics in the Twentieth Century,” *History of Science* 51, no. 2 (2013): 151–177, on 164.

well as his or her equally personal reactions to scientific fashions and the general scientific ambience.”<sup>11</sup> He continued by noting that philosophical and emotional stances towards the scale structure of the natural world directly inform the institutional structure of the sciences.

This paper responds to the need Staley identifies by exploring the dynamics of the contexts Dresden describes. The episodes reconstructed below show how the same philosophical commitments that grew from and influenced conceptual developments also played a central role in shaping the disciplines and institutions of American physics in the second half of the twentieth century. On the basis of these case studies, this project echoes Staley’s call for historians to consider scientists’ philosophical commitments when examining institutions, disciplines, and other large-scale features of science’s historical development.

## [FIRST LEVEL HEADING] BACKGROUND: POST-WORLD WAR II GROWTH OF SOLID STATE AND PARTICLE PHYSICS

Pre-World War II American physics lacked a robust sub-disciplinary infrastructure. Physicists identified as theorists or experimentalists and formed professional networks around common research interests, but these intellectual groupings had no institutional analogues. That began to change after World War II when nuclear physics, flush with the prestige of the Manhattan Project, attracted a growing proportion of young physicists and established a model

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<sup>11</sup> Max Dresden, “The Klopsteg Memorial Lecture: Fundamentality and Numerical Scales—Diversity and the Structure of Physics,” *American Journal of Physics* 66, no. 6 (1998): 468–482, on 471.

for sub-disciplinary cohesion.<sup>12</sup> Groups with common interests, conscious that both government and industry were primed to make large material investments in science, sought dedicated institutional representation.<sup>13</sup>

In 1943, as American physicists were planning their transition to peacetime, Harvard theorist John H. Van Vleck maligned the “balkanization” of American physics. He applied the term to Roman Smoluchowski’s suggestion that the American Physical Society (APS) establish a division of metals physics.<sup>14</sup> Smoluchowski, son of the Polish physicist Marian Smoluchowski, was a research physicist at General Electric.<sup>15</sup> Seeking to galvanize a community of physicists

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<sup>12</sup> David Kaiser, “Cold War Requisitions, Scientific Manpower, and the Production of American Physicists after World War II,” *HSPS* 33 (2002): 131–159 and “The Postwar Suburbanization of American Physics,” *American Quarterly* 56 (2004): 851–888, has tied this shift in the organization of American physics to pedagogical realities created by the post-World War II influx of students into graduate programs.

<sup>13</sup> Spencer Weart, “The Solid Community,” in *Out of the Crystal Maze: Chapters from the History of Solid State Physics*, ed. Lillian Hoddeson, Ernst Braun, Jürgen Teichmann, and Spencer Weart (Oxford: Oxford University Press, 1992), 617–669, and Joseph D. Martin, “Solid Foundations: Structuring American Solid State Physics, 1939–1993” (PhD dissertation, University of Minnesota, 2013) both describe the process by which a loosely affiliated group of physicists interested in the properties of solids organized efforts for representation within the American Physical Society.

<sup>14</sup> John Van Vleck to Saul Dushman, 29 Jan 1944, CRS, Box 1, Folder 1.

<sup>15</sup> The elder Smoluchowski worked predominantly in statistical physics, and today is remembered, among other accomplishments, for exorcizing Maxwell’s demon. John D. Norton,

who shared his interest in metals, he approached several colleagues to help draft a letter marshaling support for a metals division. Smoluchowski and his colleagues distributed the letter to 53 physicists nationwide.

Van Vleck was one of them. Responding to the letter, he rejected the assumption that dedicated institutional representation would protect the interests of metals physicists. Van Vleck worried that APS-sanctioned partitions would inhibit free and open communication between physicists with divergent research interests and undermine the “Quaker spirit” of the APS meetings, where physicists of all stripes could converse freely on the lawn outside the Bureau of Standards in Washington, D.C. where the APS traditionally held its annual convocation.<sup>16</sup> Smoluchowski, who hoped strong institutional structures would facilitate communication, replied to Van Vleck expressing his desire to “stir up a movement to give younger physicists a chance to meet, through Symposia, other physicists interested in the same field and at the same time to give them more voice in the Society.”<sup>17</sup> Their disagreement hinged on whether imposing organization upon the APS membership would serve their shared ideal of open scientific discourse. Smoluchowski viewed metals physicists, especially those working in industry, as marginalized. He emphasized the potential of institutional structures to give them influence within the APS. Van Vleck suspected that further institutional apparatus would break up a tight-knit community, and thought that a new division would attract industry-based metallurgists, engineers, and physical chemists, whom he did not consider true physicists. Despite Van Vleck’s

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“All Shook Up: Fluctuations, Maxwell’s Demon and the Thermodynamics of Computation,” *Entropy* 15, no. 10 (2013): 4432–4483.

<sup>16</sup> Van Vleck to Dushman (ref. 14).

<sup>17</sup> Roman Smoluchowski to John Van Vleck, 15 Mar 1945, CRS, Box 1, Folder 2.

and others' concerns, the APS Division of Solid State Physics—having been broadened from the original proposal for a division of metals physics—was established in 1947.

The nuclear and particle physics communities underwent similar processes around the same time. Nuclear physics took full advantage of the Atomic Energy Commission's National Laboratories to build a network of reactor-based research sites, parlaying the success of the Manhattan Project into enthusiastic federal patronage.<sup>18</sup> The infrastructure that resulted from the rapid growth of particle accelerators, especially stemming from Ernest Lawrence's Berkeley laboratory, gave physicists who focused on elementary particles the means, method, and opportunity to branch off from nuclear physics.<sup>19</sup> The scale of the machinery and consequent necessity of large, dedicated facilities to conduct the relevant experiments helped establish a disciplinary culture that allowed these subfields to achieve autonomy.<sup>20</sup> By the end of the 1950s, solid state, nuclear, and particle physics were firmly established.

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<sup>18</sup> Peter J. Westwick, *The National Labs: Science in an American System, 1947–1994* (Cambridge, MA: Harvard University Press, 2003).

<sup>19</sup> John L. Heilbron and Robert W. Seidel, *Lawrence and His Laboratory: A History of the Lawrence Berkeley Laboratory*, vol. 1 (Berkeley: University of California Press, 1989).

<sup>20</sup> See Laurie M. Brown, Max Dresden, and Lillian Hoddeson, "Pions to Quarks: Particle Physics in the 1950s," in *Pions to Quarks: Particle Physics in the 1950s*, ed. Laurie Brown, Max Dresden, and Lillian Hoddeson (Cambridge, UK: Cambridge University Press, 1989), 3–39 and Peter Galison, *Image and Logic: A Material Culture of Microphysics* (Chicago: University of Chicago Press, 1997). The American Physical Society's Divisions of Nuclear Physics and Particles and Fields were not established until 1967 and 1968, respectively. The groups were dominant enough within the American Physical Society in the years shortly after World War II

The growth of a sub-disciplinary menagerie encouraged direct competition for resources and prestige. For a time, government and military funds were sufficient to support most research groups and keep tensions low. The sudden leveling off of government funding in the mid-1960s, after the community had expanded so rapidly, constituted a Malthusian moment for American physics. Its new sub-disciplines, growing into adolescence, became embroiled in sibling rivalry. The following case studies indicate how those tensions interacted with the incompatible conceptions of fundamental research that matured within competing sub-disciplinary camps.

#### [FIRST LEVEL HEADING] CASE STUDY 1: FRANCIS BITTER AND THE NATIONAL MAGNET LABORATORY

In 1939, Francis Bitter articulated his vision for metallurgy in an unpublished document entitled “Abstract of the Present State and Possible Developments in Physical Metallurgy.” Bitter had earned his Ph.D. in physics from Columbia in 1928, but joined MIT in 1934 in the Department of Mining and Metallurgy, where he would remain until transferring to the Department of Physics in 1945. Between leaving Columbia and joining MIT, Bitter spent time at assorted and auspicious institutions. He conducted postdoctoral research with Robert Millikan at Caltech, worked as a research physicist for Westinghouse, and visited the Cavendish Laboratory on a Guggenheim Fellowship. During these appointments his interests evolved from his thesis

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to make due without specialized divisional representation. I. I. Rabi even maintained that divisions were only necessary for “peripheral fields.” Council of the American Physical Society, Minutes of the Meeting Held at Chicago, 25 and 26 Nov 1949, American Physical Society meeting minutes and membership lists, 1902–2003, NBL.

work on the magnetic susceptibilities of gases to the nature of ferromagnetism.<sup>21</sup> His outlook was also shaped by a pre-doctoral stint in Berlin in 1925–1926, during which he immersed himself in the unfolding quantum revolution.<sup>22</sup> Arriving at MIT fresh off his Guggenheim, Bitter was enthusiastic, and his outlook on metallurgy optimistic. He imbued his hopes for the field with a physicist’s conception of fundamental progress, articulated in his “Abstract,” that shaped his subsequent efforts to craft the National Magnet Laboratory’s mission.

Bitter described his strategy for molding metallurgy into fundamental science:

During my brief association with the subject of metallurgy I have obtained the impression that in this field more than any I have come into contact with, there is now an opportunity for rapid and fundamental development through an application of the concepts and techniques of physics and chemistry. The achievement of such progress must come as a result of the cooperative effort of a group of men whose chief interest it is to discover and

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<sup>21</sup> Ferromagnetism research in the 1930s was particularly lively, and pointed to questions of foundational importance for the subsequent development of solid state physics. See Stephen T. Keith and Pierre Quédec, “Magnetism and Magnetic Materials,” in Hoddeson, et al., *Crystal Maze* (ref. 13), 359–442.

<sup>22</sup> Francis Bitter, *Magnets: The Education of a Physicist* (New York: Doubleday & Company, Inc., 1959), 55. Bitter recalled that during his time in Berlin he heard Max Planck speak on thermodynamics, attended the colloquium at which Erwin Schrödinger introduced wave mechanics, and taught himself electricity and magnetism from Max Abraham’s textbook, *The Classical Theory of Electricity and Magnetism*. Since this text was not available in English translation at the time, Bitter most likely refers to the 1923 German edition: Max Abraham and August Föppl, *Theorie der Elektrizität* (Leipzig: B. G. Teubner, 1923).

classify the properties of metals and alloys in all their generality with the aim of formulating physical laws, rather than to follow the behavior of certain special alloy systems in detail with the aim of developing and understanding commercial processes.<sup>23</sup>

Bitter distinguished between the engineering and the scientific components of metallurgical research and found that through insufficient development of the latter, the former lacked “proper help and stimulation of a fundamental nature.”<sup>24</sup> He described how metallurgy might position itself to make what he deemed fundamental contributions, which involved building a robust conceptual foundation rooted in physics and chemistry, plus a strategy for collaborating across disciplinary boundaries to borrow techniques and insights from fields with established fundamental research programs. This grand vision for metallurgy might just as well have been a roadmap for solid state physics. It called for understanding general features of metals through theoretical physics, a focus on mechanical properties, research on crystal structure, and increased understanding of phase transitions, all of which would fall under the auspices of solid state once the field cohered after World War II.

Bitter’s understanding of fundamentality was permissive. Physics had it, chemistry had it, and metallurgy could attain it by adopting the nobler habits of these disciplines. Bitter’s optimism for the future of metallurgy required a two-stage process of fostering basic insights and then building a close relationship with practical applications, a relationship he believed drove scientific progress generally. “The physicist develops the fundamental laws which the engineer applies. In chemistry we have a similar situation,” he wrote, before presenting his rhetorical call

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<sup>23</sup> Francis Bitter, “Abstract of the Present State and Possible Developments in Physical Metallurgy,” ca. 1939, FBP, Box 5, Folder MIT Magnet Lab.

<sup>24</sup> Ibid.



to arms, asking: “Who, in metallurgy, is doing a similar job?”<sup>25</sup> Bitter emphasized that constructive dialogue between abstract science and its applications could generate advances in both, and that establishing regular discourse between the them was necessary to make the field of metallurgy a fundamental science.

Two criteria for fundamentality can be distilled from Bitter’s disquisition on metallurgy. The first, a theoretical criterion, was the formulation of general principles; the second, a practical consideration, was usefulness in a wide variety of new research. The main flaw Bitter perceived in contemporary metallurgical work was lack of emphasis on codifying regularities in the behavior of metals. The field lacked the generalizing input of theory. The hallmarks of fundamental disciplines, Bitter maintained, were theoretical principles that applied—and that actually were applied—beyond narrowly defined systems. A science concerned with the properties of metals and alloys, therefore, could become fundamental by crafting a theoretical scheme useful for describing the properties metals and alloys as a class of materials.

Organizing existing knowledge into a generalized scheme, though, was not the ultimate goal of fundamental research; fundamental science also had to prove useful in areas where knowledge was less sure-footed. Bitter emphasized fecundity as a marker of fundamentality. This term implies the actuality, not just the potential, for generating intellectual progeny: the general principles that satisfy Bitter’s first criterion can be deemed fundamental once they prove their worth in a realm for which they were not specifically designed. Characterizing Bitter’s primary criterion in this way makes clear that fecundity-based fundamentality is only recognizable once the actual influence of a piece of research is known. From this perspective, achieving fundamentality demands more than modeling research on disciplines that already

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<sup>25</sup> Bitter, “Physical Metallurgy” (ref. 23).

exhibit it; it requires building the personal and institutional relationships through which research efforts can exercise real influence. Science becomes fundamental, in the fecundity sense, when those relationships have born fruit by successfully provoking new research and serving as a foundation for new conclusions.

Bitter perceived this quality in physics and chemistry. Both sciences aimed to formulate general principles, but more importantly, rich interactions between inquiry and applications drove progress in these sciences and made them broadly applicable to fields like metallurgy and engineering. For Bitter, the problem was not just that metallurgists wanted for a robust theory of metals; they were not even in dialogue with people who were working to develop one. In the absence of such an interaction, Bitter thought, metallurgy would be limited to cataloguing and quantifying the properties of a growing alloy zoo. This type of research, because it did not pursue general laws or ask novel questions, could never be fundamental. His remedy was to encourage metallurgists to overcome the insularity of their field and collaborate with physicists and chemists to build the bridges that would foster new thinking. Bitter's recommendations called for MIT metallurgists to change the way they organized their department and fit within Institute infrastructure, suggesting, for example, that "one or two physicists in the metallurgy department ... carry out their work in close contact with the rest of the staff," and promoting "closer contact with [John C.] Slater's work in Physics and with the work of [Charles W.] MacGregor in Mechanical Engineering."<sup>26</sup>

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<sup>26</sup> Ibid. The appellation "fundamental" was commonly employed permissively around this time. Bitter was in accordance with the accepted usage by suggesting that sciences other than physics could be fundamental. Frederick Seitz, writing just a few years later, identified "fundamental" with "pure" research, defining it as that "which has intrinsic value as a form of culture." He

Generality, fecundity, and interdisciplinary collaboration—the pillars of Bitter’s view of fundamental knowledge—were pivotal to the founding and growth of the National Magnet Laboratory two decades later. Bitter coordinated planning for the NML, which opened in 1960 as a facility dedicated to high magnetic field research. Its early history reflects Francis Bitter’s 1939 vision. The proposal that convinced the Air Force Office of Scientific Research to fund the lab framed its mission compatibly with Bitter’s notion of fundamentality, its goal being “to make continuous fields up to 250,000 gauss available for fundamental research in solid state and low temperature physics and related fields, and to serve as a center for advancing the art of field generation.”<sup>27</sup> By 1960, Bitter’s had transferred to the physics department and he built the NML

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further mirrored elements of Bitter’s definition by suggesting that “physics serves as a source of fundamental knowledge for a majority of the most important fields of engineering.” Frederick Seitz, “Whither American Physics?,” *Review of Scientific Instruments* 16, no. 2 (1945): 39–42, on 40. Vannevar Bush asked rhetorically in the inaugural issue of *Physics Today*, “who would have expected, looking forward from, say, 1939, to find the United States Navy vigorously furthering a program in fundamental science, including nucleonics, genetics, and mathematics?” Vannevar Bush, “Trends in American Science,” *PT* 1, no. 1 (1948): 5–7, 39, on 6. Sabine Clarke has demonstrated that “fundamental research” took on a range of meanings in the first half of the twentieth century that did not obey the basic/applied distinction. Sabine Clarke, “Pure Science with a Practical Aim: The Meanings of Fundamental Research in Britain, circa 1916–1950,” *Isis* 101, no. 2 (2010): 285–311.

<sup>27</sup> “Proposal for a High Field Magnet Laboratory,” 8 Sep 1958, NMLR, Box 1, Folder 55.

with solid state physics, rather than metallurgy, in mind.<sup>28</sup> The NML would support foundational research in order to enrich the field and make it more productive. A quote from Bitter's laboratory dedication speech, composed long before budget concerns caused NML staff to emphasize its practical offshoots, indicates the place he saw it occupying within the scientific community: "The solid-state research program is being transferred from the M.I.T. magnet laboratory to the new facility [the NML]. The aim of this program is to increase knowledge of the basic electrical, magnetic, optical, acoustical, and thermal properties of solids. This fundamental information, pursued for its own sake, has and will continue to provide the basis for the continuing development of new and improved solid-state electronic devices."<sup>29</sup> Concern with establishing an environment in which fundamental research could flourish was at the forefront of Bitter's thinking. With the NML, he institutionalized his convictions about fundamental research, hoping that its structure would encourage research that could serve as the basis for something more.<sup>30</sup>

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<sup>28</sup> Bitter took a leave of absence from MIT to work on degaussing naval ships during World War II. During this time, his metallurgical magnetism laboratory was dismantled and its resources redistributed to war work. Upon his return to MIT at the end of the war, both he and the administration thought it more appropriate to reassemble the magnetism program under the auspices of the physics department.

<sup>29</sup> Francis Bitter, "Dedication of the National Magnet Laboratory," 30 Apr 1963, FBP, Box 5, Folder NML Dedication Notes.

<sup>30</sup> The extent to which it was possible in practice for an installation such as the NML to be truly devoted to basic research while operating on military funding is a matter of some debate. See: Paul Forman, "Behind Quantum Electronics: National Security as Basis for Physical Research in

[Fig. 1 about here]

Administrative responsibility was shared among the departments that used the NML. Beyond offering a venue for research using high magnetic fields, the facility provided an interdepartmental forum for MIT scientists and engineers and attracted visiting researchers from other institutions, again in accordance with Bitter's view that fundamental work should be outward looking. The NML was also an educational space. Bitter had admonished the metallurgical community in 1939 that fundamental advances required training students to ask fundamental questions. In the early 1960s, the NML promoted itself as just such an opportunity for MIT graduate students. A 1963 brochure emphasized this aspect of the lab's mission, touting the "opportunity to pursue extremely fundamental speculations" graduate students enjoyed, citing work on magnetic field dependence of the velocity of ultrasonic waves in metals one

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the United States, 1940–1960," *HSPS* 18 (1987): 149–229; Stuart W. Leslie, *The Cold War and American Science: The Military-Industrial-Academic Complex at MIT and Stanford* (New York: Columbia University Press, 1993); Joan Bromberg, "Device Physics vis-a-vis Fundamental Physics in Cold War America: The Case of Quantum Optics," *Isis* 97 (2006): 237–259; and Benjamin Wilson, "The Consultants: Nonlinear Optics and the Social World of Cold War Science" *Historical Studies in the Natural Sciences* (this issue). Forman and Leslie argue that military interest diverted—or at least inclined—Cold War solid state research away from fundamental work. In contrast, Bromberg and Wilson argue that applied military research co-existed, and indeed interacted constructively, with fundamental theoretical work. I do not take a position on this debate here. Though I am sympathetic to the case Bromberg and Wilson advance, it is enough here that a desire to pursue basic research manifested itself in the laboratory's goals and operations.

doctoral student was pursuing.<sup>31</sup> By 1965, the lab's second full year of operation, its 13 academic staff supported 24 students, who worked alongside 9 researchers from Lincoln Laboratory—which MIT administered—and 56 visiting scientists.<sup>32</sup>

The NML provided the space, resources, community, and pedagogical opportunities necessary for a solid state research facility embodying Bitter's vision. Bitter, who was nearing the end of his career, did not take an active role in the lab's administration. Benjamin Lax became its first director. Lax was Hungarian-born, but had immigrated to the United States in his youth and earned his bachelor's degree at Cooper Union in 1941. He was drafted while pursuing doctoral work at Brown and arrived at MIT in 1944 as a Radiation Laboratory researcher. He stayed on after the war, completed his Ph.D. in 1949, and joined the Lincoln Laboratory shortly thereafter, rising to head of the Solid State Division in 1958.

The selection of Lax as NML director represented the desire to coordinate solid state research across departments. John Slater, who wielded significant influence as an Institute Professor and head of the physics department's solid state and molecular theory group, wrote to MIT President Julius Stratton as plans for the NML were brewing: "I feel that if we seized the opportunity presented by the Magnet Laboratory, if it goes through, and correlated it with [...] work on solids in the departments of Physics, Chemistry, Electrical Engineering, and some of that in Metallurgy, we should have the possibility of building up a solid-state laboratory of great value not only to M.I.T. and the educational program, but to the services and the country as a

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<sup>31</sup> National Magnet Laboratory promotional brochure, 1963, FBP, Box 5, Folder NML Dedication Notes.

<sup>32</sup> "Visiting Scientists and Students," 1965, NMLR, Box 2, Folder 17.

whole.”<sup>33</sup> Slater advocated grouping representatives from these departments “together with Lax and as much of the Lincoln solid-state group as could possibly be included, in a great cooperative organization, if possible housed together on or close to the campus, and including both students, professors, and research scientists of the Lincoln type.”<sup>34</sup> Support from figures of Slater’s prominence ensured that the new lab, beyond absorbing the existing magnet program Bitter had established, would seek greater integration of solid state work across MIT’s campus.

With institutional support for interdepartmental collaboration secured, Lax carried Bitter’s ethos forward. “I believe it is important for us to provide in the field of basic solid state and applied physics, centers of excellence that will contribute to education and to science in a most effective way,” he wrote to Roman Smoluchowski in 1965.<sup>35</sup> The NML was productive. By December 1965, its thirty-eight staff members had published sixty-one papers that calendar year, with twenty-nine more in press, accepted, or submitted, and had collectively delivered seventy-nine meeting or colloquium talks. These numbers more than doubled 1963 totals, far outstripping the 18.75 percent staff increase in the same interval.<sup>36</sup> Of the 61 articles, book chapters, and monographs published in 1965, more than half, thirty-seven, were published in American Institute of Physics journals. Of these, nineteen appeared in the *Physical Review* or *Physical*

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<sup>33</sup> John C. Slater to Julius A. Stratton, 8 Aug 1958, FBP Box 3, Folder High Field Magnet Facility, No. 1 of 3.

<sup>34</sup> Ibid.

<sup>35</sup> Benjamin Lax to Roman Smoluchowski, 10 Mar 1965, NMLR, Box 3, Folder 25.

<sup>36</sup> “NML Publications Record,” 31 Dec 1965, NMLR, Box 2, 17. From 1963 to 1965 staff increased from 32 to 38.

*Review Letters*, the flagship publications dedicated to basic physical research.<sup>37</sup> These articles contributed to major contemporary solid state research trajectories, for instance by examining the band structure of solids and properties of superconductors. Although crude, these data indicate that a large proportion of the lab's output was dedicated to the type of foundational work Bitter championed.

The facility was a strong draw for young talent within the solid state community. Lab director Benjamin Lax found himself with an embarrassment of riches in the mid-1960s and complained to the National Research Council's Solid State Sciences Panel that despite having "interviewed a greater fraction of first-rate young scientists than we have throughout my entire career ... with very few exceptions, we reluctantly turned these away."<sup>38</sup> Lax himself won the Oliver E. Buckley Prize in 1960, which, although less than a decade old at that point, was among the most prestigious accolades a solid state physicist could garner. He would be elected to the National Academy of Sciences in 1969. It was not for want of results that the Air Force's enthusiasm for the facility began to wane in the mid-1960s.

In 1965 the NML administration asked the National Science Foundation (NSF) to take over financial responsibility for visiting scientist support and a share of both magnet maintenance and research costs from the Air Force. Funding for the laboratory had stalled after

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<sup>37</sup> The remainder were spread over the *Journal of Applied Physics* (8), *Review of Scientific Instruments* (4), *Applied Physics Letters* (2), and one each in the *Journal of Chemical Physics*, *Journal of Mathematical Physics*, *Physics of Fluids*, and *Physics Today*.

<sup>38</sup> Benjamin Lax to Roman Smoluchowski, 10 Mar 1965, John C. Slater Papers, American Philosophical Society, Philadelphia, PA, Folder National Academy of Science-National Research Council, Solid State Sciences Panel.



its initial ramp-up. Widespread national shortages in basic science funding, coinciding with the escalation of the Vietnam War, became a prod with which to nudge the lab towards a more explicitly applied stance. Lax noted with some alarm the Air Force's increased interest in the applicable fruits of magnet research in 1967: "This is a complete change from the past when NML was discouraged from including in its charter an applied program."<sup>39</sup> Lloyd A. Wood, director of the physical sciences division within the Air Force's Office of Scientific Research, substantiated this observation, writing to Lax a month later: "It is as you know becoming more and more an issue in Washington to 'couple' federally supported basic research to 'practical' enterprises, and a large project such as the Magnet Laboratory has a great opportunity for doing this."<sup>40</sup>

An eye towards applied benefits was not incompatible with the NML's stated mission, which had, since Bitter's early vision, emphasized the importance of basic insight for technological advance. "Coupling" of basic research funding with explicitly practical considerations, however, challenged Lax's vision for the lab and he resisted any reorientation of the NML's core mission. He was happy to accommodate an overtly applied program so long as the Air Force was willing to supply the requisite funding, but maintained: "Financially we are in no position to begin such work on our own."<sup>41</sup> Lax also pushed to keep applied projects and their funding isolated from the operations and basic research budgets. He testified before Congress on the transition from Air Force to NSF funding, for instance, that the NML "has always coupled its basic research results with the mission-oriented agencies having the greatest interest in a

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<sup>39</sup> Benjamin Lax to George H. Vineyard, 17 Mar 1967, NMLR Box 2, Folder 18.

<sup>40</sup> Lloyd A. Wood to Benjamin Lax, 19 Apr 1967, NMLR, Box 2, Folder 18.

<sup>41</sup> Lax to Vineyard (ref. 39).

particular line of development and will continue to do so,” while qualifying that commitment by saying, “[w]hen there is a development of special interest to an agency we will solicit support and participation by that agency, whether it be the Air Force or other DOD department, NASA, NIH, or the environmental agencies.”<sup>42</sup> Explicitly applied projects were fine, in Lax’s eyes, so long as they did not divert attention or funding from the fundamental work he considered the lab’s *raison d’être*.

As negotiations with the NSF continued through the mid-1960s, the laboratory faced tight budgets and an uncertain future. The NML advisory committee was initially agitated. In February 1966, the committee struck a defiant tone in the face of restrictive budgets, maintaining “the strong opinion that a moderate and orderly expansion of funding is desirable,” and further noting: “It is discouraging and unhealthy for a Laboratory, after a vigorous initial period of building up from zero to a viable state, to be abruptly leveled off by budgetary constraints, when large areas of interesting and appropriate research remain.”<sup>43</sup> Just over a year later, the committee was more resigned to the difficult environment. An April 1967 report offered less resistance to

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<sup>42</sup> Lax to the Subcommittee on Science, Research and Development of the Committee on Science and Astronautics, 5 Mar 1971, NMLR, Box 2, Folder 4. Lax does use “coupling” language here, but is careful to write that the results of basic research can be coupled with applied questions, rather than to the planning, execution, or funding of the research.

<sup>43</sup> “Report of the Advisory Committee of the National Magnet Laboratory,” Feb 1966, NMLR, Box 2, Folder 17.

budgetary stasis, and resignedly noted: “similar budget freezes affect all solid state physics research, if not most scientific research activities in this country at the present time.”<sup>44</sup>

Lax was not so content to accept the lab’s struggles just because they were symptomatic of larger trends. He vented his frustration in a letter to a fellow solid state physicist, Harvard’s Nicolaas Bloembergen: “It is true that there is a budget squeeze all throughout the country, particularly on solid state. However, as it turns out, funds have been found for other areas of physics which are already better funded overall nationally than the solid state activities at the universities. This, in spite of the fact that solid state constitutes by far the largest segment of the physical society.”<sup>45</sup> Lax was similarly candid in a letter to National Science Foundation director Leland Haworth, calling it “preposterous ... that the country’s only national facility for high magnetic field research is hamstrung while millions are being spent on redundant facilities in

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<sup>44</sup> “Report of the Advisory Committee of the National Magnet Laboratory,” Apr 1967, NMLR, Box 2, Folder 18.

<sup>45</sup> Benjamin Lax to Nicolaas Bloembergen, 10 May 1967, NMLR, Box 2, Folder 18. In 1967 the Division of Solid State Physics had 1193 members, compared to 762 in the Division of Nuclear Physics, the next-largest division. The Division of Particles and Fields held its inaugural meeting in January 1968 with a charter membership of 551. W. V. Smith to Division of Solid State Physics Members, 6 Jan 1967, John C. Slater Papers, American Philosophical Society, Philadelphia, PA, Folder American Philosophical Society, #5; “Proceedings of the American Physical Society Meeting #425,” 1967, American Physical Society meeting minutes and membership lists, 1902–2003, NBL; M. Davis to Chairmen and Secretary-Treasurers of APS Divisions, 15 Feb 1968, American Physical Society Records, Subgroup 2, NBL, Box 17, Folder 10.

other scientific disciplines.”<sup>46</sup> By “redundant facilities,” Lax almost certainly had in mind the National Accelerator Laboratory (NAL)—better known as Fermilab—plans for which the Atomic Energy Commission had approved just months earlier. Lax, the director of a large, one-of-a-kind solid state research facility was irked that accelerator laboratories were proliferating while his own was being forced to curtail its programs.

In what Lax considered a serious concession, the NML’s work did shift in a more applied direction towards the end of the 1960s. In 1968, the number of publications by NML staff in the *Journal of Applied Physics* (18) equaled the combined total of those published in *Physical Review* and *Physical Review Letters*.<sup>47</sup> An April 1969 Advisory Committee report noted the addition of a program designed to explore medical applications of magnetic fields.<sup>48</sup> In the early 1970s, the lab initiated more aspirational applied projects, such as the magneplane, which endeavored to translate the NML’s knowhow into magnet-powered railroad system. The magneplane was a particularly sore point for Lax, who felt it epitomized the concessions the NML had made to Vietnam-era demands for applied payouts from solid state facilities pursuing basic research. On more than one occasion, NML research scientist Henry Kolm, who headed the project, clashed with Lax’s over the lab’s mission.

In a memo to Lax entitled “Magnetism Applications Projects,” Kolm described himself as “the only strong-minded SOB who has survived in your entourage,” and voiced his frustration

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<sup>46</sup> Benjamin Lax to Leland Haworth, 10 May 1967, NMLR, Box 2, Folder 18.

<sup>47</sup> “Publications of the Francis Bitter National Laboratory in 1968,” NMLR, Box 2, Folder 20. In 1965, by contrast, the number of publications in the latter two journals more than doubled those in the former.

<sup>48</sup> “Meeting of the Advisory Committee,” Apr 1969, NMLR, Box 2, Folder 20.

with Lax's disapproval of Kolm's applied interests: "Our magnetism applications programs are not a concession to expediency, an act of prostitution in the bleak years of 69 to 71. They are a long-neglected obligation of the scientific community. They are giving new relevance to our graduate education, revitalizing our professional stature, and improving the survival chances of the laboratory, of MIT, and of the entire scientific establishment."<sup>49</sup> Kolm objected to Lax's contention that the raft of applied projects the lab had acquired syphoned funds from its mission-critical research. In a stark indication of the depth of their disagreement of the NML's mission and direction, Kolm suggested: "if you find it impossible to integrate a significant applications program into the 'core' work of the laboratory in such a way that you and others do not resent its existence, then serious consideration should be give [sic] to severing it administratively [...] by creating a new laboratory."<sup>50</sup>

No such schism was forthcoming, but the tensions between Lax and Kolm reveal the extent to which changes in federal science policy challenged the NML's mission. Lax, who administered in accordance with Bitter's vision of fundamental research, was shaken by the need to take on applied projects for their own sake. Kolm, representing a younger generation, was less ideologically opposed to adding applied objectives to the laboratory's mission.

By 1971 the NML—renamed the Francis Bitter National Magnet Laboratory in November 1967, following Bitter's death—had found more stable, if not more generous, financial support from the NSF. Its struggles through the late 1960s and early 1970s are telling: Francis Bitter's view of fundamentality, although realizable in a major research laboratory, did not fare as well in the larger funding environment. The Air Force, its enthusiasm for facilities

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<sup>49</sup> Henry Kolm to Benjamin Lax, 2 Jun 1973, NMLR, Box 2, Folder 32.

<sup>50</sup> Ibid.

devoted to non-applied work dwindling, began transferring responsibility to a civilian agency, limiting the lab's expansion, well before the 1973 Mansfield Amendment compelled such a transfer on a larger scale.<sup>51</sup>

As the NML struggled, particle accelerators thrived. Unlike solid state physicists, who could not justify a large facility without invoking potential practical outcomes, particle physicists reaped large-scale expenditures based on the promise of fundamental knowledge, as future NAL director Robert R. Wilson famously did in 1969 when he told Congress that the proposed accelerator “has nothing to do directly with defending our country except to make it worth defending,” and insisted that the search for fundamental physical knowledge provided the same culturally ennobling qualities as art and literature.<sup>52</sup>

Particle physicists, ambivalent over military and economic justifications for their research in the face of 1960s protest movements, widely embraced the high-minded rhetoric of fundamentality that Wilson's congressional testimony epitomized.<sup>53</sup> They were successful in casting particle physics as “a grand cultural enterprise, elegant and profound, that deserved the support of society.”<sup>54</sup> That avenue to funding, especially large-scale government funding, was

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<sup>51</sup> See Daniel J. Kevles, *The Physicists: The History of a Scientific Community in America* (Cambridge, MA: Harvard University Press, 1995), esp. 420–421 and Glen R. Asner, “The Linear Model, the U.S. Department of Defense, and the Golden Age of Industrial Research,” in *The Science-Industry Nexus: History, Policy Implications*, ed. Karl Grandin, Nina Wormbs, and Sven Widmalm (Sagamore Beach, MA: Watson Publishing International, 2004), 3–30.

<sup>52</sup> Quoted in Stevens, “Fundamental Physics” (ref. 8), on 174.

<sup>53</sup> Ibid.

<sup>54</sup> Ibid., 175.

not available to solid state physicists, who by the 1960s were already too closely associated with technology in the imaginations of federal funders. The permissive view of fundamentality around which Bitter had build the NML failed to fill the lab's coffers. Five-year plans and annual reports to the Air Force stressing the NML's fundamental contributions could not replicate the rhetorical success of Wilson's eloquent testimony on behalf of the NAL. Faced with this failure, the NML's solid state physicists felt slighted by the comparatively more severe funding shortfalls they suffered and resented the emerging perception in the particle physics community that fundamental knowledge could only be derived from the ultimate constituents of matter and energy. These frustrations extended beyond the walls of the NML. At the dawn of the 1970s, solid state physics was a mature discipline, confident in its ability to generate fundamental scientific knowledge, but it was in the midst of an identity crisis exacerbated by unfavorable comparisons to its more lauded siblings.

#### [FIRST LEVEL HEADING] CASE STUDY 2: "MORE IS DIFFERENT"

Philip W. Anderson, a Bell Laboratories solid state theorist, was also perturbed by the financial difficulties basic solid state research faced. Anderson had completed his Ph.D. with John Van Vleck in 1949. He immediately joined the solid state group at Bell where he imbibed the spirit of the Bell system, in which, after winning the favor of his supervisor, William Shockley, he could pursue his interests almost unencumbered.<sup>55</sup> Bell was still buzzing from the

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<sup>55</sup> Lillian Hartmann Hoddeson, "The Roots of Solid-State Research at Bell Labs," *PT* 30, no. 3 (1977): 23–30 describes the process by which a basic research ethos took root at Bell Labs, and

invention of the transistor two years earlier, which promised to revolutionize the telecommunications industry. This and other high-profile successes attracted a slew of talented young physicists and Anderson found himself among a uniquely large and accomplished assemblage of solid state theorists and experimentalists. Stefan Machlup, a postdoc who overlapped with Anderson, wrote to Shockley to recount his impressions and recalled, “I think everybody on my hall was doing exactly what he wanted to do,” and expressed awe at the concentration of expertise, observing that although “it’s not as ‘gemütlich’ [cozy] as a college campus ... [i]f you’ve got an obscure technical problem, chances are that somewhere in the two-mile network of corridors sits *the* expert on this particular specialty.”<sup>56</sup> Through the 1950s and 1960s, benefiting from Bell’s vibrant intellectual climate, Anderson conducted the research that would earn him the Nobel Prize he shared with the University of Bristol’s Neville Mott and his advisor, John Van Vleck, in 1977, “for their fundamental theoretical investigations of the electronic structure of magnetic and disordered systems.”<sup>57</sup>

[Fig. 2 about here]

By the late 1960s, however, even denizens of the Bell oasis could detect twinges of the concern abroad in the solid state community. In the spring of 1967 Anderson delivered a lecture at the University of California, San Diego that formed the seed of his 1972 *Science* article,

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chronicles how the establishment of solid state research in this context led to the *laissez faire* approach distinctive to the major American industrial laboratories of the time.

<sup>56</sup> Stefan Machlup to William Shockley, 30 May 1955, William Shockley Papers, Stanford University Archives, Palo Alto, CA, Box 2, Folder Correspondence 1954 and 1955.

<sup>57</sup> “The Nobel Prize in Physics 1977,” Nobelprize.org, [http://www.nobelprize.org/nobel\\_prizes/physics/laureates/1977/](http://www.nobelprize.org/nobel_prizes/physics/laureates/1977/) (accessed 7 Dec 2011).



“More Is Different.” The talk grew, as Anderson later recalled, from the simmering discontent he perceived within the solid state community.<sup>58</sup> In Anderson’s view, nuclear and particle physicists monopolized influential government advisory roles, many prominent universities hired solid state faculty only as an afterthought, and solid state physicists had trouble breaking into prestigious institutions such as the National Academy of Sciences (NAS). The NAS member rolls validate Anderson’s concern. Between Charles Kittel in 1957 and both Anderson and Charles Slichter in 1967, the Academy admitted six solid state physicists compared with twenty-seven nuclear and particle physicists.<sup>59</sup>

Anderson was also responding to the particle physics community’s reductionist view of fundamentality, in particular to Victor Weisskopf’s distinction between intensive and extensive research (Fig. 3).<sup>60</sup> Weisskopf, who had served on Anderson’s doctoral examining committee, identified two camps in the physics community in a 1967 *Physics Today* article; “intensivists,” he claimed, sought first principles above all else, while “extensivists” ferreted out useful

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<sup>58</sup> Philip W. Anderson, “More Is Different – One More Time,” in *More Is Different: Fifty Years of Condensed Matter Physics*, ed. N. Phuan Ong and Ravin N. Bhatt (Princeton: Princeton University Press, 2001), 1–9.

<sup>59</sup> National Academy of Sciences, “Members,”

<http://www.nasonline.org/site/Dir?sid=1011&view=basic&pg=srch> (accessed 10 Feb 2011).

Data taken from both current and deceased member rolls. The National Academy of Sciences database does not indicate deceased members’ sections. They were counted only if they could be identified as unambiguous examples of solid state, nuclear, or particle physicists.

<sup>60</sup> Anderson, “One More Time” (ref. 58), 1–2.

applications of those principles, either to technology or to other scientific fields.<sup>61</sup> The intensivist position, as Weisskopf described it, aptly characterized the view of fundamentality particle physicists adopted in the 1960s.<sup>62</sup> Similarly, his description of extensive research captured one aspect of the type of fundamentality Bitter championed, with the caveat that Weisskopf would not have called that type of research fundamental.

[Fig. 3 about here]

Weisskopf cited Alvin Weinberg—not to be confused with particle theorist Steven Weinberg—as a paradigm extensivist. Alvin Weinberg, a nuclear physicist by training, wrote a *Physics Today* article in 1964 warning of tough choices ahead as funding for scientific research plateaued. As the director of Oak Ridge National Laboratory, he would be charged with deciding which projects to pursue and which to sideline when funding tightened. Anticipating this challenge, Weinberg proposed criteria for determining which research could claim priority in funding battles. He echoed Bitter’s view of fundamental research as work that provides a foundation for further developments, articulating what would become known as the Weinberg

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<sup>61</sup> Viktor Weisskopf, “Nuclear Structure and Modern Research,” *PT* 20, no. 5 (1967): 23–26, on 24.

<sup>62</sup> The assumption that general principles are worked out in one realm before being applied in another is also prominent within the history of physics. It has contributed to the impression that solid state physics was a field devoted to mere applications of more fundamental work. Jeremiah James and Christian Joas challenge this assumption in “Subsequent and Subsidiary? Rethinking the Role of Applications in Establishing Quantum Mechanics,” *Historical Studies in the Natural Sciences* (this issue). They argue that so-called applications of early quantum mechanics made many essential contributions to the foundations of the theory.

Criterion: “The word ‘fundamental’ in basic science, which is often used as a synonym for ‘important,’ can be partly paraphrased into ‘relevance to neighboring areas of science.’ I would therefore sharpen the criterion of scientific merit by proposing that, other things being equal, *that field has the most scientific merit which contributes most heavily to and illuminates most brightly its neighboring scientific disciplines.*”<sup>63</sup> Conceptual fecundity was only one element of Weinberg’s fundamentality calculus. His view included not only conceptual content, but also technological fruitfulness and social relevance as components of fundamentality. Tested against these criteria, Weinberg rated particle physics as poor, writing, “I know of few discoveries in ultra-high-energy physics which bear strongly on the rest of science.”<sup>64</sup>

Weinberg pushed this agenda vigorously at the 1964 meeting of the APS in Washington, arguing: “Science which commands great public support must be justified on grounds that originate *outside* the particular branch of science demanding the support; it must rate high in social, technological, or scientific merit, preferably in all three,” and challenging particle physicists “to state their case clearly, to say exactly why it is that elementary-particle physics is as important as all our elementary-particle physicists believe it is. To say that it is ‘fundamental’

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<sup>63</sup> Alvin Weinberg, “Criteria for Scientific Choice,” *PT* 17, no. 3 (1964): 42–48, on 45.

<sup>64</sup> *Ibid.*, 47. Terminological note: physicists grew to prefer “high energy” and “condensed matter” to “particle” and “solid state” later in the century. For consistency I use the older terms throughout. Important difference exist in each set of terms, but they overlap enough that each pair can be treated as continuous for the purposes of this argument. For more on the differences between solid state and condensed matter, see: Joseph D. Martin, “What’s in a Name Change? Solid State Physics, Condensed Matter Physics, and Materials Science,” *Physics in Perspective* 17, no. 1 (2015): 3–32.

in itself does not answer the question, because one then has to decide what one means by ‘fundamental.’ I tried to interpret the idea ‘fundamental’ in basic science to mean having the greatest kind of bearing on the rest of science, and even on other human knowledge.”<sup>65</sup>

Such a stance holds clear utility for the director of a large, broadly invested laboratory such as Oak Ridge. Weinberg valued research programs that would promote useful connections with other enterprises within the laboratory or shore up the laboratory’s social and political support.

Weinberg’s relational approach, which focused on the interconnectedness of knowledge, set him in direct opposition to the emerging orthodoxy within particle physics. His disconnect with Weisskopf is evident in a co-authored *Physics Today* article entitled “Two Open Letters,” published June 1964. Weisskopf argued that “the nucleon is the basis for all matter and therefore of all science,” whereas Weinberg retorted that he was nonetheless, “justified in characterizing high-energy physics as ‘rather remote.’”<sup>66</sup> Weisskopf emphasized the possibility in principle of reducing higher-level laws to lower, but Weinberg focused on the impracticality of doing physics from the bottom up. Weisskopf claimed that theories of lower levels were privileged because higher-level theories could be reduced to them; Weinberg allowed any field that met his fecundity criteria to claim privilege. It did not matter for Weinberg, when choosing which

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<sup>65</sup> C. N. Yang, E. L. Goldwasser, Val Fitch, A. M. Weinberg, and Owen Chamberlain, “High-Energy Physics: Round-Table Discussion,” *PT* 17, no. 11 (1964): 52, 57.

<sup>66</sup> Viktor Weisskopf and Alvin Weinberg, “Two Open Letters,” *PT* 17, no. 6 (1964): 46–47, on 47. The quark model was proposed by Murray Gell-Mann and George Zweig in 1964, but did not gain experimental traction until 1967, so Weisskopf’s identification of nucleons as the basis for all matter is, in content and rhetoric, equivalent to later claims of the same status for the standard model and its imagined successors.

research to fund, whether or not higher-level phenomena could, in principle, be explained in terms of lower-level phenomena after the fact because, contra Weisskopf, he tested the degree of fundamentality research exhibited by its relationship to other areas of knowledge rather than by its purported relationship to physical reality. These difference account for why Weisskopf and Weinberg talked past each other: they disagreed at the core about how fundamentality was derived

Anderson's entry into this discussion did not, as might seem natural, mirror Weinberg's argument that fundamental research is that which exhibits broad relevance. Instead, he rejected Weisskopf's narrow characterization of intensive research. Anderson's "More Is Different" marks a transition in the form the debate took. He adopted a permissive view of fundamentality, like Bitter and Weinberg, but he abandoned the fecundity argument. Anderson accepted one of the particle physicists' terms of debate. He agreed with his reductionist opponents that fundamentality was innate to some types of scientific knowledge. He disagreed with them about the realms in which it could be found. Despite differences with his allies, he did attempt to ground their primary conclusion—that reductionist physics should not be funded to the detriment of other fields—on a sound philosophical basis. In doing so he focused the debate around a single disagreement about the nature of scientific knowledge.

"More Is Different" begins: "The reductionist hypothesis may still be a topic for controversy among philosophers, but among the great majority of active scientists I think it is accepted without question."<sup>67</sup> Some biologists, chemists, psychologists, or even other solid state physicists might have resisted this characterization, but the statement did reflect prevailing trends

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<sup>67</sup> Philip W. Anderson, "More Is Different," *Science*, New Series 177, no. 4047 (1972): 393–396, on 393.

in the physics community. In 1970, particle physics research received approximately four government dollars for every one spent on basic solid state research.<sup>68</sup> This was despite the fact that the American Physical Society's Division of Solid State Physics remained the largest division and was over twice as large as the Division of Particles and Fields.<sup>69</sup> The reductionist hypothesis—even if it was not, as Anderson claimed, naïvely accepted within the scientific community broadly—was reflected in the way the federal government funded physics.

Anderson opposed the reasoning, which he attributed to particle physicists, “that if everything obeys the same fundamental laws, then the only scientists who are studying anything really fundamental are those who are working on those laws.”<sup>70</sup> This view, according to Anderson, started from the hypothesis that laws and concepts operating on any given level of complexity could be reduced to laws and concepts at a lower level of complexity. It thereby concluded that only research addressing the ultimate constituents of matter and energy could be truly fundamental. The reductionist argument built into particle physicists' philosophy of scientific knowledge the justification for pursuing research on progressively smaller scales, using accelerators of progressively higher energy and greater cost. By arguing that the foundational character of smaller physical scales conferred privilege upon knowledge of the laws governing

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<sup>68</sup> National Research Council *Physics in Perspective*, vol. 2, pt. A (Washington, DC: National Academy of Sciences, 1972), 129, 453. Tables I.6 and IV.1 of this report show \$211.7 million in total expenditure for particle physics versus \$56 million for basic condensed matter research.

<sup>69</sup> Untitled document, American Physical Society Records, Subgroup 2, NBL, Box 17, Folder 10. Data taken from numbers collected following a 1968 membership drive.

<sup>70</sup> Anderson, “More Is Different” (ref. 67), 393.

those scales, reductionism supported the view that science funding should reflect the hierarchy particle physicists saw in the physical world.

To undermine this position, Anderson moved past the Weinberg Criterion and its emphasis on conceptual, technological, and social applicability. He accepted the standard reductionist premise that laws governing higher-level phenomena could be reduced to laws governing lower-level phenomena, but rejected the inverse, claiming that the particle physicists' view of fundamentality rested on a constructionist hypothesis, which assumed that higher-level laws could be extrapolated from lower-level laws. With this move, Anderson attacked the narrow definition of intensive research that underwrote the reductionist scientific hierarchy, preserving the ability of solid state physicists to claim fundamental insight, contending: "The main fallacy of this kind of thinking is that the reductionist hypothesis does not by any means imply a 'constructionist' one: The ability to reduce everything to simple fundamental laws does not imply the ability to start from those laws and reconstruct the universe."<sup>71</sup> Anderson's emergence-based view of fundamentality granted the concepts and laws of solid state physics independence by virtue of the fact that they could not realistically be derived from lower-level concepts and laws alone.

He illustrated his point with the example of an ammonia molecule, aiming to demonstrate construction's failure even at the level of simple molecular systems. Naïvely, ammonia should have a dipole moment, given its pyramidal structure's asymmetry. A nitrogen atom forms polar covalent bonds with three hydrogen atoms, leaving the nitrogen with a net negative and the hydrogen with a net positive charge. The resulting tetrahedron, though, does not empirically behave like a dipole. In its stationary state, the molecule is in a superposition of the left hand and

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<sup>71</sup> Ibid.

right hand orientations; when observed through time it undergoes a process of inversion, in which the nitrogen atom tunnels through the triangle of hydrogen atoms and emerges on the other side, several billion times per second.

A broad swath of physicists would have been familiar with ammonia's properties; it provided the material basis for the original maser, which Charles Townes and his research group had announced, to considerable acclaim, in 1955.<sup>72</sup> Nitrogen inversion itself had been a familiar chemical process for several decades. It received renewed interest in the mid-1950s when nuclear magnetic resonance spectroscopy allowed it to be measured with accuracy superior to that provided by older radio-frequency techniques.<sup>73</sup> Anderson leveraged a familiar example to illustrate the implausibility of applying foundational symmetry laws to larger systems without reference to higher-level structure: "I would challenge you to start from the fundamental laws of quantum mechanics and predict the ammonia inversion and its easily observable properties without going through the stage of using the unsymmetrical pyramidal structure, even though no 'state' ever has that structure."<sup>74</sup> Construction, in other words, is impracticable: "The relationship between the system and its parts is intellectually a one-way street. Synthesis is expected to be all but impossible; analysis on the other hand, may not only be possible but fruitful in all kinds of ways," a result he hoped would undercut "the arrogance of the particle physicist" and allow that

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<sup>72</sup> J. P. Gordon, H. J. Zeiger, and C. H. Townes, "The Maser—New Type of Microwave Amplifier, Frequency Standard, and Spectrometer," *Physical Review* 99, no. 4 (1955): 1264–1274.

<sup>73</sup> See: John D. Roberts, *Nuclear Magnetic Resonance: Applications to Organic Chemistry* (New York: McGraw-Hill, 1959), 74–76.

<sup>74</sup> Anderson, "More Is Different" (ref. 67), 394.



“each level can require a whole new conceptual structure.”<sup>75</sup> Anderson did not draw out this statement’s implications, but it is tempting to see the tacit suggestion that a whole new conceptual structure would require a whole new funding structure.

The distinction between impossible in practice and impossible in principle lingers under the surface of Anderson’s analysis. He rejected the restrictive view of intensive research, and yet hedged when saying that synthesis of lower-level laws to find higher-level laws is “*all but impossible*” and calling it an “intellectual,” rather than a physical or natural one-way street. He claimed that the laws of solid state physics could never *practically* be extrapolated from quantum mechanics without reference to empirically established, higher-level phenomena; he fought shy of the stronger claim that higher-level laws could never *in principle* be derived from below.

Anderson does not explain the delicate dance he executes by linking the independence of higher-level concepts to practical rather than physical considerations. I propose four reasons this position is notable. The first two reflect Anderson’s more general focus on the practice of science. The third and fourth draw on other elements of his context and help explain how “More Is Different” fit within it. I do not reject the possibility that Anderson quite straightforwardly considered the argument that new levels were independent in practice to be the better-justified

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<sup>75</sup> Ibid., 396. This argument has parallels familiar to philosophers of biology; it is a common anti-reductionist argument that biological processes do not make sense in terms of genes alone, and that reference to the organismal level, at least, is required to explain phenomena such as differential fitness. This position is astutely summarized by humorist Douglas Adams, who observes: “If you try to take a cat apart to see how it works, the first thing you have on your hands is a nonworking cat.” Douglas Adams, *The Salmon of Doubt: Hitchhiking the Galaxy One Last Time* (New York: Random House, 2002), 135–136.

position. Rather than providing an exhaustive account of why Anderson held the view he did, the factors discussed below demonstrate how richly interconnected Anderson's position was with the professional context. Anderson has himself acknowledged that his perspectives were shaped by conditions in the scientific community that were unfavorable to solid state physicists:

"Sociologists of science posit that there is a personal or emotional subtext behind much scientific work, and that its integrity is therefore necessarily compromised. I agree with the first but reject the second. I think 'More Is Different' embodies these truths. The article was unquestionably the result of a buildup of resentment and discontent on my part and among the condensed matter physicists I normally spoke with."<sup>76</sup> As a result, considering how the fine structure of Anderson's argument worked within the context that motivated it can be instructive.

The first reason Anderson's focus on practical level independence is notable is that it reflects his more general focus on scientific practice. Here, he represents continuity with Bitter and Weinberg. Permissive views of fundamentality were uniformly hardheaded about the process of scientific research. The in-principle derivability of higher-level phenomena was useless if it provided no practical directives for doing science. Anderson reprised his argument in 2001: "a perverse reader could postulate a sufficiently brilliant genius—a super-Einstein—who might see at least the outlines of the phenomena at the new scale; but the fact is that neither Einstein nor Feynman succeeded in solving superconductivity."<sup>77</sup> Similarly, Anderson's ammonia example

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<sup>76</sup> Anderson, "One More Time" (ref. 58), on 1.

<sup>77</sup> Ibid., 4. Superconductivity was a notoriously intractable theoretical puzzle. Failing to derive a theory of it was almost a rite of passage for the most accomplished theoretical physicists before John Bardeen, Leon Cooper, and Robert Schrieffer succeeded in 1957. Felix Bloch famously advanced the theorem that all theories of superconductivity can be disproven, a refinement of

drew its force from the fact that anyone attempting to describe an ammonia molecule's behavior for the first time would, by any reasonable understanding of practice, be required to employ higher-level concepts in addition to so-called first principles.

Second, that necessity supplied a font of new solid state problems. In a 1999 interview with Alexei Kojevnikov, Anderson recalled being motivated to develop his philosophical views in part by a lecture Brian Pippard, a Cambridge solid state theorist, had given at a superconductivity conference hosted by IBM in 1960.<sup>78</sup> Pippard lamented a lack of compelling and accessible fundamental problems in solid state, suggesting that solutions to the most prominent—such as superconductivity—had deprived the field of appealing intellectual challenges for young talent. Pippard offered up the gloomy prognostication that “ten years is going to see the end of our [solid state physicists’] games as pure physicists, though not as technologists,” and advocated “a swing of emphasis now away from pure research to applications,” which should necessitate exposing promising students to “the methods of research in industrial laboratories.”<sup>79</sup> Anderson, who spent the 1967–1968 academic year as Pippard’s colleague during a visiting professorship at Cambridge, described him as “a professional pessimist.”<sup>80</sup> The second axis of Anderson’s argument from practice is evident in his reaction

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Wolfgang Pauli’s more cutting version of the theorem: theories of superconductivity are wrong. See: Jörg Schmalian, “Failed Theories of Superconductivity,” in *BCS: 50 Years*, ed. Leon N. Cooper and Dmitri Feldman (Singapore: World Scientific, 2011), 41–56.

<sup>78</sup> Philip W. Anderson, interview by Alexei Kojevnikov on 23 Nov 1999, NBL, [http://www.aip.org/history/ohilist/23362\\_3.html](http://www.aip.org/history/ohilist/23362_3.html) (accessed 14 Nov 2011).

<sup>79</sup> Brian Pippard, “The Cat and the Cream,” *PT* 14, no. 11 (1961): 38–41, on 40–41.

<sup>80</sup> Anderson, interview (ref. 78).

against Pippard's pessimism regarding academic solid state research. "More Is Different" makes the case for the widespread availability of academy-friendly, intellectually interesting basic research problems in solid state physics. The practical necessity of employing higher-level concepts to describe solid state systems provided, for Anderson, a nearly inexhaustible supply of new and interesting fundamental questions. The failure of construction ensured that solving longstanding problems did not impoverish the field so much as Pippard supposed; surprising physics could always be expected when considering the next level of complexity. This interpretation meshes well with Anderson's clear preference for practical considerations, because the question of in-principle independence had little bearing on whether an adequate supply of interesting research problems would be available to slake the intellectual thirst of future graduate students.

Third, the narrow focus on practice was expedient. A strong claim about the nature of objective physical reality was not essential to allow solid state research a claim to fundamental knowledge given an argument that denied the practical possibility of synthesis. Because claims to fundamental knowledge and financial support were correlated during this period, at least for particle physicists, Anderson can be read as making the weakest claim necessary to advance his position without inviting attack from those who objected to the wholesale independence of higher levels from lower levels. So long as the case could be made for *acquiring knowledge* of higher levels, questions of *physical* hierarchy were merely academic.<sup>81</sup>

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<sup>81</sup> Cat, "Physicists' Debates on Unification" (ref. 7), makes a similar observation when assessing Anderson's position in terms of the unity of physics. Anderson, according to Cat, saw physics as methodologically (rather than ontologically) unified. If physics could be unified by

Finally, the contours of Anderson's philosophical position and the consequences it had for fundamentality debates can be understood in terms of changing prestige politics in the late 1960s and early 1970s. Physics enjoyed considerable prestige following World War II, but as the funding plateau Weinberg predicted arrived, prestige, like financial support, became a limited resource, leaving physicists to carve it up among their sub-disciplines. Separating reduction and construction severed the link between physical hierarchy and intellectual prestige. Anderson sought to deny particle physicists an exclusive claim to fundamental knowledge, knowing, as Weinberg did, that "fundamental" often meant the same thing as "important." The weaker position also avoided the question of whether a permissive view undermined the prestige of physics more generally. Solid state might have been struggling in this period, but physics was still firmly established as the standard bearer for American science. A stronger view implying the equivalence of all scientific knowledge would have been strange given the circumstances. Cat's analysis is useful here when considering the relationship between physics and other sciences. By avoiding strong claims about the ultimate nature of reality, Anderson shifted the focus to methodology as a basis by which physics could be at once internally unified and delineated from other sciences.<sup>82</sup> Although Anderson gives no indication that this was a conscious motivation, his argument does have the convenient consequence of undermining the exclusive claim particle

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methodology, rather than by the reduction to a single set of laws and concepts, then fundamental physical knowledge need not be restricted to the lowest level of complexity.

<sup>82</sup> Ibid.

physicists laid on fundamental knowledge without similarly undermining the more general entitlement physicists felt to rare levels of social approbation and federal funding.<sup>83</sup>

Examination of these contextual pressures brings Anderson's departure from Bitter and Weinberg into focus. The difference between his views and his predecessors' corresponds to a shift in conditions within the scientific community. The funding pinch in the late 1960s and early 1970s was asymmetrical. Particle physics, the flagship enterprise of reductionism, enjoyed continued success in the form of new, expensive facilities. Anderson recognized the growing prestige and funding gaps between solid state and particle physics. Amid these conditions, which became more acute as large government grants became more difficult to obtain, Anderson developed a view of fundamentality that departed sharply from fecundity arguments, although it arrived at similar conclusions. Given the undistinguished, impecunious position Anderson perceived solid state physics to occupy in the late 1960s and early 1970s, the well-worn claim that research needed only to provide a basis for further research to be fundamental would not

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<sup>83</sup> Anderson subsequently expanded his view to encompass concepts in the social and biological as well as the physical sciences, but recalled that his initial sensitivity to the level-dependence of concepts arose because it was "the principle by which my own field of science arose from the underlying laws about particles and interactions; and it was only as I broadened my perspective that I realized how general emergence is." Philip W. Anderson, "Emergence vs. Reductionism," in *More and Different: Notes from a Thoughtful Curmudgeon* (Singapore: World Scientific, 2011), 134–139, on 135. It is therefore appropriate to understand Anderson's 1972 stance as primarily about physics, even though he later refined it into a view about science in general. It is also worth noting that by the time Anderson's emergentism had fully matured, biology had unseated physics as the doyen of the American sciences.

have met the challenge particle physics posed. Instead, Anderson sought the source of fundamentality in the nature of physical knowledge, adopting the strategy that had served particle physicists so well. “More Is Different” makes the best historical sense when placed against the foil of particle physics and its strong reductionism and set within a context where physics still dominated American science. The fecundity notion of fundamentality implied no hierarchy and did not play favorites among the sciences. Anderson, by accepting the innate view of fundamentality more typical of the reductionist account, denied particle physics an exclusive claim to the privilege physical knowledge enjoyed, as a matter of course, over chemical, biological, or social scientific knowledge.

Solid state physicists had been concerned with keeping disciplines like chemistry at a safe distance since the 1940s. Among those most concerned with maintaining that boundary was Anderson’s Ph.D. advisor, John Van Vleck. In 1944, Van Vleck, in the same exchange with Roman Smoluchowski during which he lamented the Balkanization of physics, wrote: “The idea that various groups whose main interest is not physics must be coddled, in order to make them members of the American Physical Society, has never appealed to me.”<sup>84</sup> Smoluchowski did not object to the sentiment, but replied that forming an entity to advocate for the interests of metals physics did not constitute an effort to bring other fields under the umbrella of physics, but rather “an attempt to prevent ‘an invasion and partition by powerful neighbors’ (i.e. chemists and metallurgists).”<sup>85</sup> Changes in the physics during in the 1960s and early 1970s gave solid state physicists the inverse worry: they felt isolated from the center of the physics community. Anderson responded by developing a philosophy aimed at halting that process by reaffirming

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<sup>84</sup> John Van Vleck to Saul Dushman, 29 Jan 1944, CRS, Box 1, Folder 1.

<sup>85</sup> Roman Smoluchowski to John Van Vleck, 3 Feb 1944, CRS, Box 1, Folder 1.

solid state's contributions to the conceptual core of physics.

“More Is Different” pursued this goal by arguing the negative case that fundamentality was not unique to particle physics. Anderson gave examples of non-reductionist physics that he believed provided fundamental contributions, but his positive account of what it means to be fundamental was less developed. Max Dresden, in 1974, described more fully a positive program compatible with Anderson's position. Dresden, best known to historians of physics for his biography of Hendrik Kramers, was a polymath within the physics community.<sup>86</sup> He was a Dutch-born émigré, who arrived in the United States in the late 1930s and earned his Ph.D. from the University of Michigan in 1946 with a dissertation in statistical physics. His career was peripatetic; he hopped from the University of Kansas to Northwestern University and then to the University of Iowa before settling at the State University of New York at Stony Brook in the mid-1960s. Both geographically and conceptually, Dresden developed a broad perspective. In addition to moving between many institutions, Dresden was able to turn his acumen with statistical methods to a plethora of problems in many subfields of physics. By the time he rendered his verdict on reductionism, he was familiar with the conceptual and professional issues both particle and solid state physicists faced.

Unlike Anderson, Dresden confronted the in principle versus in practice question head on. He argued that physical descriptions at different scales were, in principle, autonomous from descriptions of their substrata on the basis of the distinctness of the concepts employed at higher

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<sup>86</sup> Max Dresden, *H. A. Kramers: Between Tradition and Revolution* (New York: Springer, 1987).

The example of Dresden is also considered by Cat, who focuses on Dresden's attack on reduction as a relationship between mathematical descriptions at successive levels. Cat, “Physicists' Debates on Unification” (ref. 7).



levels of description. Considering how the foundational concepts of solid state physics—such as lattice structures, energy bands, free electrons, phonons, and electron-phonon interactions—relate to the supposedly “more fundamental” description of solid state systems in terms of component parts and their interactions, Dresden argued:

The level in which the analysis is carried out, really defines the scale – and the autonomy of the scales and the lack of any simple relationship between the scales, follows directly from the manner in which they were obtained. This means that every level of description has to develop its own concepts and methods, for organizing and structuring its information. In this process reference to underlying, deeper levels, to universal results of those levels might be useful and important. But ... the search for regularities and concepts, especially adapted to the level under study can not exclusively (or primarily) be based on a *deductive* analysis of the deeper levels. Thus the *independence* of the scales is every bit as important as their interrelations.<sup>87</sup>

Dresden’s discussion complements Anderson’s by emphasizing the impossibility of deducing the higher-level concepts that constitute the medium of scientific exchange in solid state physics from first principles.

Like others who sought, in his words, a “less partisan” account of fundamentality, Dresden focused on the creation of scientific knowledge rather than *post hoc* rationalizations of its place within a unifying scheme.<sup>88</sup> He took a stronger stand than Anderson did on the in-

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<sup>87</sup> Max Dresden, “Reflections on ‘Fundamentality and Complexity,’” in *Physical Reality and Mathematical Description*, ed. C. P. Enz and J. Mehra (Boston: D. Reidel, 1974), 133–166, on 158.

<sup>88</sup> *Ibid.*, 134.

principle independence of higher-level phenomena. Dresden did not defend the claim that physics would remain more fundamental than other disciplines given the autonomy of higher-level concepts, but he limited his discussion to physical considerations, discussing chemistry only to show that molecules provided examples of physical complexity.<sup>89</sup> Discussions such as Anderson's and Dresden's reflected the tensions between different branches of physics, and, while many of the views they advanced had consequences beyond that realm, those consequences were pushed to the background, as even physicists with permissive views of fundamentality accepted the field's privileged place in American science.

Anderson's and others' refinements of earlier conceptions of fundamentality suggest how changing financial realities and corresponding hierarchical shifts within the scientific community exerted pressure on scientists to develop philosophical stances. Solid state physicists'

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<sup>89</sup> Dresden mirrors Anderson's avoidance of chemistry, biology, and other fields in his 1974 discussion. Also like Anderson, he gives them a larger role in subsequent articulations of his position. In 1997, Dresden delivered the Paul Klopsteg Memorial Lecture before the American Association of Physics Teachers and spoke about the necessity of scale-specific concepts in science and the failure of "the fundamentalist credo" that the universe is fully describable in terms of "laws and concepts which completely specify the interactions between the irreducible objects." Dresden, "Fundamentality and Numerical Scales" (ref. 11), 469. On page 479 of the same article, Dresden left his narrow focus on physics behind and asserts: "It is [...] a rather common occurrence that as the number of constituents or degrees of freedom  $N$  increases, new concepts become relevant. This is especially true in biology and chemistry." He concluded the article with a discussion of levels of organization in chemistry and biology and argues that the diversity complex scales entail is an essential driving force in the sciences generally.

fecundity-based view of fundamentality was well adapted to the early post-World War II funding environment. Changes in the environment and growth of a competing philosophy in the form of virulent reductionism among particle physicists motivated an extension and sharpening of their position, leading physicists to place their convictions on more philosophically rigorous ground. To whatever extent Anderson's views on fundamentality might have been based in his research, he was moved to refine and articulate them by professional challenges.

### [FIRST LEVEL HEADING] CASE STUDY 3: THE SUPERCONDUCTING SUPER COLLIDER

The previous two case studies have explored how individuals' convictions about fundamentality manifested in the establishment and administration of a laboratory and how a changing professional environment motivated some physicists to advance pointed philosophical arguments. These examples, alone or in tandem, are insufficient to show how philosophical considerations played out on the level national priorities and large-scale funding decisions. The third case study, which focuses on the Superconducting Super Collider (SSC), indicates how philosophical commitments shaped interactions between the scientific community and the political entities that supported it, and how physicists reimagined those commitments as a result. The current literature provides a detailed accounting of the factors that led to the SSC's demise.<sup>90</sup>

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<sup>90</sup> See Daniel J. Kevles, "The Death of the Superconducting Super Collider" in *The Physicists: the History of A Scientific Community in Modern America*, 3rd ed. (Cambridge, MA: Harvard University Press, 1995), ix–xlii, which discusses the changes wrought by the end of the Cold War and the rise of an unsympathetic freshman congressional delegation; Michael Riordan, "A Tale of Two Cultures: Building the Superconducting Super Collider, 1988–1993," *HSPS* 32, no.

This case study does not aim to replicate such efforts, but instead to examine the SSC as an example of how physicists' philosophical debates intensified, changed, and probed the limits of their influence when they entered a more public context.

By the mid 1980s, when the SSC project was gathering inertia, both the restrictive and permissive concepts of fundamentality were established doctrine for the particle and solid state communities, respectively. The SSC, in its very concept, was the apotheosis of the reductionist commitment to exploring the energy frontier in search of ultimate physical truth. As reductionist sentiment peaked among particle physicists, a sizable segment of the solid state community had begun to rebrand themselves “condensed matter physicists,” emphasizing “the discoveries of fundamental new phenomena and states of matter, the development of new concepts, and the opening up of new subfields,” the investigation of complex matter generated.<sup>91</sup>

Each of these perspectives would be challenged, and deformed, during the grueling cross-examinations the SSC and its mushrooming price tag endured in both congressional committees

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1 (2001): 125–144, which examines dysfunction within the SSC's management structure; and Lillian Hoddeson and Adrienne W. Kolb, “The Superconducting Super Collider's Frontier Outpost, 1983–1988,” *Minerva* 38 (2000): 271–310, which exposes the early disconnect between the vision of the physicists designing the SSC and the inclinations of those commanding the federal purse strings.

<sup>91</sup> National Research Council, *Condensed-Matter Physics: Physics through the 1990s* (Washington, D.C.: National Academy Press, 1986), 3. I have argued elsewhere that “condensed matter physics” was meaningfully different from “solid state physics in some contexts,” though for the purposes of this paper the philosophical views expressed within these two traditions were continuous. Martin, “Name Change” (ref. 64).

and the court of public opinion. Particle physicists articulated the strongest forms of reductionism they could muster in order to justify expenditures on physics, which, on the face, was remote from quotidian social, economic, and national security concerns. Simultaneously they embellished the standard reductionist justification with spin-off claims in an attempt to meet the unambiguous congressional demand for return on investment. Solid state physicists also tailored their views as they tried to balance Anderson's claim for the innate intellectual value of complex concepts with Alvin Weinberg's argument for funding physics on the basis of demonstrated conceptual, social, and technological relevance.

Although solid state physicists and particle physicists had their squabbles in the 1960s and 1970s, the hegemony of physics within the American scientific community remained intact through these decades. By 1993, when Congress cut off funding to the SSC, biology was ascendant.<sup>92</sup> The Human Genome Project, launched in 1990, represented the new face of big science; physics no longer held the unchallenged excess of scientific prestige and political influence it had enjoyed earlier in the century. Furthermore, the end of the Cold War and economic hiccups of the early 1990s—which Kevles has detailed—eroded the national patience for large expenditures, whose payoffs lay beyond the horizon.<sup>93</sup> Shifts in the prevailing national goals in the early 1990s led both to the extravagant spin-off claims high-energy physicists made while trying to justify the SSC, and to a resurgence of the fundamentality-as-fecundity view

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<sup>92</sup> See Daniel J. Kevles, “Big Science and Big Politics in the United States: Reflections on the Death of the SSC and the Life of the Human Genome Project,” *HSPS* 27, no. 2 (1997): 269–297, which describes this transition by comparing the failure of the SSC with the success of the Human Genome Project.

<sup>93</sup> Kevles, “Death of the SSC” (ref. 90).

among solid state physicists, who felt renewed pressure to promote the relevance of solid state work to other areas of science, technology, and society.

The philosophical disputes of the 1960s and 1970s had remained internal to the scientific community. The corresponding conflicts in the late 1980s and early 1990s expanded from the confines of *Science* and *Physics Today* to the floor of Congress and the pages of the popular press. When the formerly arcane dispute became a matter of public policy and the implications for the future of physics in the United States became clearer, physicists voiced their views more forcefully and to more varied audiences. At the same time, the currency those views carried diminished among scientists who were forced to contend with myriad other concerns that populated the legislative milieu. The combination of shifted social priorities and the transition to a larger stage led both solid state and particle physicists to reformulate their views.

Spin-off claims from particle physicists represented one clear rhetorical shift. Many SSC advocates, including SSC Director Roy Schwitters, perceived that high-minded declarations about the ennobling nature of fundamental knowledge would not be sufficient to convince legislators that the project was worthwhile. “Elementary particle physics does not exist in isolation,” Schwitters asserted in his written statement for a 1989 meeting of a Senate’s Subcommittee meeting on the Department of Energy budget: “Stimulation, information, and techniques flow both ways: from other activities toward particle physics, and from particle physics toward other activities.”<sup>94</sup> He emphasized intellectual overlap with nuclear physics, cosmology, and solid state, and stressed that the demands of accelerator engineering had

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<sup>94</sup> Senate, Hearing before Subcommittee on Energy, Research, and Development Committee on Energy and Natural Resources, *Proposed Fiscal Year 1990 Budget Request (DOE's Office of Energy Research)*, 101st Cong., 2nd sess., 24 Feb 1989, 100.

technological knock-on effects for computing, superconducting magnets, and semiconductor devices. The cutting edge technical needs of accelerators, Schwitters maintained, “provide fruitful interchange with other researchers, manufacturers, and developers of technology in fields such as medical imagery.”<sup>95</sup> Such claims aimed to establish that particle physics could be fundamental in a relational sense and an innate sense.

Fecundity arguments and reductionist arguments were sometimes conflated even more directly. Cosmologist George Smoot, testifying in 1992 before a hearing on the “Importance and Status of the Superconducting Super Collider,” recounted an anecdote about sharing a plane with a group of cataract patients traveling for laser surgery only available in the United States, which he used to illustrate the claim: “It just shows you do not know what development is going to turn out to be something useful.... You really have to understand the basics of all science. That is where physics, and particularly higher energy physics comes together because physics is what we call the queen of sciences. It is the basic underlying structure for all of sciences, the foundation everything sits on. You have to understand physics to understand what is going on.”<sup>96</sup> The rhetorical strategy that tied the supposed generative power of particle physics to its status as the science of the fundamental scale proved controversial, even among SSC advocates.

Smoot, for example, testified immediately after Leon Lederman, who had won a Nobel Prize in 1988 for his neutrino research. Lederman gave legislators a more conventional version

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<sup>95</sup> Ibid., 102.

<sup>96</sup> Senate, Joint Hearing before the Committee on Energy and Natural Resources and the Subcommittee on Energy and Water Development of the Committee on Appropriations, *On the Importance and Status of the Superconducting Supercollider*, 102nd Cong., 2nd sess., 17 Jan 1992, 27.

of reductionist rhetoric. He employed the strategy, popular among particle physicists during the hearings, of casting the SSC as the culmination of a narrative beginning in Ancient Greece: “The road from Miletis [sic] to the SSC is what philosophers call a reductionist road. . . . Until we can complete the unification process and make the picture mathematically whole, the question of how the world works will not be answered.”<sup>97</sup> He dissociated the reductionist justification from spin-offs claims: “[Technological benefits] would be a crazy reason to build the SSC. We do not build it for the spin-offs. We build it because we are humans who think and are insatiably curious, and have an unquenchable determination to know,” recalling Robert Wilson’s apology for the National Accelerator Laboratory 25 years earlier.<sup>98</sup>

Lederman’s argument contrasts the willingness of his younger colleagues to adopt fecundity arguments when convenient. The split might well have been generational. Schwitters and Smoot were both born in the mid-1940s and earned their doctorates in the early 1970s. In contrast Lederman, born in 1922, along with another advocate for the unembellished reductionist justification, Steven Weinberg, born in 1933, were among the generation who had overseen the articulation of the philosophy in the 1960s. For them, stooping to arguments on the basis of technological output weakened the justification for pursuing fundamental physics for its own sake. The high stakes of the SSC debates influenced these two groups differently. On one hand, it promoted an intensification of the reductionism that had underwritten particle physics’ push to higher energies through the 1960s and 1970s. At the same time, suspicions that such a

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<sup>97</sup> Ibid., 24. The misspelling of “Miletus,” the Greek city that was home to Thales and is widely considered to have been the cradle of Greek science and philosophy, is attributable to a transcription error rather than to Lederman himself.

<sup>98</sup> Ibid., 25–26.



justification would not work on its own prompted younger physicists to advance the fecundity argument in the form of spin-off claims, which Lederman found unnecessary.

Although divided about how spin-off claims meshed with fundamentality claims, particle physicists—and cosmologists, who were close allies of the SSC throughout the hearings—were uniformly of the opinion that the SSC was valuable because it could offer fundamental knowledge where other facilities, and other branches of physics, could not. They maintained the belief, summarized by S. S. Schweber, that “elementary particle physics has a privileged position, in that the ontology of its domain and the order manifested by that domain refer to the building blocks of the higher levels.”<sup>99</sup> Steven Weinberg provided the most impassioned defense of the strong reductionist position. The 1979 Nobel Prize had recognized his work unifying electromagnetism and the weak nuclear force, and he was a visible public advocate for reductionist science, writing a popular book that expressed optimism for the culmination of physics with a unified physical theory.<sup>100</sup> Throughout the hearings he presented justifications for building the SSC alongside a broader view of science. “We are at the frontier,” he testified in August 1993 in a hearing before the Committee on Energy and Natural Resources and the Subcommittee on Energy and Water Development of the Committee on Appropriations; “we have pushed the chain of questions why as far as we can, and as far as we can tell we cannot make any progress without the super collider.” He continued: “Well, who cares? You know, there are a lot of people, a lot of Americans, a lot of members of Congress who really see science only in terms of its applications. And that is a respectable view. Not everyone is turned on by the

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<sup>99</sup> Silvan S. Schweber, “Physics, Community, and the Crisis in Physical Theory,” *PT* 46, no. 11 (1993): 34–40, on 40.

<sup>100</sup> Steven Weinberg, *Dreams of a Final Theory* (New York: Pantheon Books, 1992).

same things. Not everyone likes classical music. Not everyone has this hunger to know why the world is the way it is, and we have to live with that. I find it sad, myself, but that is the way people are.”<sup>101</sup> By identifying those who cared only about applications as the SSC’s main opponents, Weinberg ignored the objection Anderson and others mounted that particle physics was not the only route to fundamental insight. Weinberg’s testimony throughout the SSC hearings grew from the assumption that “without this machine we simply cannot continue the process of uncovering nature’s fundamental laws.”<sup>102</sup> His claim was not only that the United States should fund the SSC because it provided fundamental knowledge, but because it was the only route to fundamental knowledge; everything else was derivative. By the early 1990s, the state of the art in reductionism was substantially more virulent than it had been in the 1960s and 1970s.<sup>103</sup> Victor Weisskopf considered particle physics the science most fully directed towards

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<sup>101</sup> Senate, Joint Hearing before the Committee on Energy and Natural Resources and the Subcommittee on Energy and Water Development of the Committee on Appropriations, *Department of Energy’s Superconducting Super Collider Project*, 103rd Cong., 1st sess., 4 Aug 1993, 51.

<sup>102</sup> House, Hearing before the Committee on Science, Space, and Technology, *The Superconducting Super Collider Project*, 103rd Cong., 1st sess., 26 May 1993, 58.

<sup>103</sup> The extremity of the reductionism espoused in the context of the SSC debates has been emphasized by Barbara L. Whitten, who argues that “Lederman and [Sheldon] Glashow, firmly entrenched in the reductionist paradigm and totally unable to hear what the others are saying, resemble straw men constructed by feminist critics to display arrogant androcentric science at its most glaring.” Barbara L. Whitten, “What Physics Is Fundamental Physics? Feminist

fundamental principles, but he saw it as occupying one extreme of smooth scale rather than as a categorically unique enterprise.

[Fig. 4 about here]

The psychological effectiveness of Steven Weinberg's dichotomy between fundamental/pro-SSC views and applied/anti-SSC views became evident when the committee chairman, Senator J. Bennett Johnston, Jr. (D-LA), introduced the next witness: "a distinguished Nobel laureate. Professor Philip P. Anderson from the Department of Physics, I think that is Applied Physics, at Princeton University."<sup>104</sup> Departing from his prepared testimony, Anderson offered two corrections for the record: "Senator Johnston and this committee, and the Democratic National Committee are the only two people who give me the middle initial 'P' when my actual middle initial is 'W'. And I receive a lot of mail from the Democratic National Committee to Phil P. Anderson. And I am not an applied physicist. I like to call myself a fundamental physicist as well. I just am fundamental in somewhat different ways."<sup>105</sup> Anderson endeavored to undermine Weinberg's hard and fast distinction between fundamental physics and applied physics, just as he had sought to undermine Weisskopf's distinction between intensive and extensive research two decades earlier. Johnston's initial error provided him an opportune segue into that argument.

Anderson's opposition to the SSC was two pronged: First, he advocated a permissive form of fundamentality that opposed particle physicists' attempts to justify the SSC on a

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Implications of Physicists' Debates over the Superconducting Supercollider," *NWSA Journal* 8, no. 2 (1996): 1–16, on 10.

<sup>104</sup> Senate, *Superconducting Super Collider Project* (ref. 101), 57.

<sup>105</sup> Ibid.

reductionist basis; second, he argued for the need to establish scientific priorities, much as Alvin Weinberg had in 1964. By introducing the second line of argument, Anderson departed from “More Is Different,” where the question of funding remained in the background. His SSC testimony repurposed his arguments about the character of scientific knowledge to underwrite a picture of how science funding should be organized. Like Steven Weinberg, his testimony transcended the SSC and pushed a perception of science that had broader implications. Anderson did not oppose the SSC *per se*, but objected to the consolidation of financial support for non-applied research in big-budget particle physics installations while solid state, confined to smaller labs, pursued narrow, practical objectives and found scant opportunity for intellectual curiosity. The narrow focus was a consequence, he believed, of the conflation of fundamentality with reduction. He described fundamental research in solid state as, “caught between the Scylla of the glamorous big science projects like the SSC, the genome, and the space station, and the Charybdis of the programmed research, where you have deliverables, where you are asked to do very specific pieces of research aimed at some very short-term goal.”<sup>106</sup>

Anderson attempted a precarious traverse of this rhetorical Strait of Messina. He advocated funding fundamental research for its own sake and opposed funding for preconceived, near-term technological outcomes. He opposed the SSC because, a) solid state was just as fundamental as particle physics, and b) funding exploratory solid state research with no strings attached would, as a matter of course, produce more socially and technologically valuable results. Solid state could boast sterling technological bona fides, but advocating funding priority over the SSC strictly on that basis would undercut Anderson’s mission to demonstrate solid state’s intellectual merit. Even if a technological justification would have fallen more musically

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<sup>106</sup> Senate, *1990 Budget Request* (ref. 94), 134.

on many legislators' ears, solid state's fight for intellectual prestige was too strong a component of its identity for Anderson to sell it short.

Anderson parsed his objections to the SSC in terms of how particle physics fit into his broader view of science. Before the House of Representatives Task Force on Defense, Foreign Policy and Space in 1991 he delivered the same message he would give the Senate:

The standard testimony you will receive on behalf of the SSC will tell you that in some sense elementary particle physics, high-energy physics is the bellwether of the sciences, the one which is out there leading the pack, the one which in some sense is investigating the 'deepest' layers of reality in the world around us and the 'most fundamental' laws of physics. ... [T]here is at least one other kind of frontier in the physical sciences where a lot of action—and I would argue more action—is taking place: the frontier of looking at bigger and more complex aggregates of matter which often behave in new ways and according to new laws. These new laws don't contradict the laws the elementary particle people discover; they are simply independent of them, and I would argue they are in no way any less or any [more] fundamental.<sup>107</sup>

Given the expense of particle physics, Anderson's testimony continued, what does it contribute in proportion to the funds it receives? How many scientists does it employ? Does the knowledge it produces either motivate practical applications or stimulate research in other areas of science? The answers to these questions should, in his view, determine how science is funded.

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<sup>107</sup> House, Hearing before the Task Force on Defense, Foreign Policy and Space of the Committee on the Budget, *Establishing Priorities in Science Funding*, 102nd Cong., 1st sess., 11 and 18 Jul 1991, 64. The word "more," which was omitted in the transcript, can be reasonably inferred.

Like particle physicists, who were compelled to make dubious spin-off claims to justify the SSC, solid state physicists were also wrenched into some uncomfortable contortions by the demands of a Congress eager to cut spending. For Anderson, this meant dulling his subtle argument of “More Is Different” and embroidering his stance with elements of the Weinberg Criterion. The position Anderson articulated in 1972 still grounded his opposition to the SSC, but his arguments in the 1990s were blunter. He claimed that laws governing complex systems were independent rather than not derivable. He leaned heavily on social and technological relevance in a way he had not in 1972. Anderson’s views evolved in response to developments in the organization of science. The pressures of reforming his philosophy for a public audience forced him to argue simultaneously for the intellectual autonomy of solid state physics and for its social and technological merits, a combination he delicately packaged as a single, coherent view.

Weinberg and Anderson each had an agenda that transcended the SSC and they spoke for their respective communities. Each used his agenda to frame his testimony. Congress therefore heard competing views of scientific research and its goals as much as they heard arguments for or against the SSC.<sup>108</sup> That more was at stake than a single research facility is evident from the extent to which the SSC debate spilled over into the popular press. Both Weinberg and Lederman published popular books espousing a reductionist view of science.<sup>109</sup> Weinberg took his case to the pages of the *New York Times* in March 1993, as the SSC’s prospects grew dire. In an op-ed

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<sup>108</sup> Ironically, the one point where they agreed—the importance of funding basic research for its own sake—was the very point to which Congress was least likely to be sympathetic.

<sup>109</sup> Steven Weinberg, *Dreams of a Final Theory* (New York: Pantheon Books, 1992); Leon M. Lederman and Dick Teresi, *The God Particle: If the Universe is the Answer, What is the Question?* (New York: Houghton Mifflin Company, 1993).

entitled “The Answer to (Almost) Everything” Weinberg appealed to a popular audience with an example about the weather, writing: “Elementary particle physics is more fundamental than, say, meteorology, not because it will help us predict the weather, but because there are no independent principles of meteorology that do not rest on the properties of elementary particles.”<sup>110</sup>

Weinberg’s article angered solid state physicists, and some shot back. Northwestern physicist Pulak Dutta retorted in a letter to the editor: “The public relations triumph of particle physics is that it has cast itself as the sole heir of atomic physics and quantum mechanics, and thus irrefutably ‘fundamental.’ However ... we’ve known for some time that elementary particles are made of quarks, but that hasn’t made (and isn’t likely to make) any difference to any other area of human activity. It just isn’t fundamental to anything.”<sup>111</sup> Dutta’s rehearsal of Alvin Weinberg’s criterion for scientific choice in the face of Steven Weinberg’s on-the-nose brand of reductionism reflected the mood among solid state researchers. They viewed particle physicists as demanding extraordinary resources while maligning the intellectual merit of solid state physics and making unrealistic spin-off claims for a field with little measurable importance to other areas of science and few prospects for making socially or technologically useful contributions.

[Fig. 5 about here]

Though reductionist rhetoric had helped secure Fermilab’s funding in the 1960s, permissive, relational fundamentality held more sway with the 1990s Congress—even if it was secondary to the broader shift in which biology replaced physics as the marquee American

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<sup>110</sup> Steven Weinberg, “The Answer to (Almost) Everything,” *New York Times*, 8 Mar 1993.

<sup>111</sup> Pulak Dutta, “Supercollider Science Is out on a Limb,” *New York Times*, 15 Mar 1993.

science. Congressman Sherwood Boehlert (R-NY), Chairman of the House Committee on Science, Space, and Technology, opened a May 1993 hearing by rehearsing arguments solid state physicists had presented: “My first concern is the basic question of priorities. SSC supporters like to suggest that to oppose the SSC is to oppose science. Nothing could be further from the truth. Science is not some indivisible domain but is made up of separate, if related, disciplines.”<sup>112</sup> Similarly, in the Senate, Senator Dale Bumpers (D-AR) brusquely dismissed the argument that particle physics and basic science were one and the same: “The assumption that anybody who opposes this project is opposed to basic science is a distraction and a diversion.”<sup>113</sup> By 1993, the arguments solid state physicists had presented to Congress over the preceding years were being echoed in the House and the Senate by Republicans and Democrats alike. The competing view solid state physicists offered allowed legislators to oppose a major scientific budget item without appearing to be anti-science.

In *Physics Today*, the widely distributed news magazine published by the American Institute of Physics, discussions of the unity of physics paralleled the debate that played out in Congressional testimony and popular writings.<sup>114</sup> In a letter published in March 1991, Lawrence Cranberg, a retired University of Virginia physicist who studied the physics of wood as it applied to domestic energy needs, wrote that particle physicists showed little interest in work not directly related to their own. He charged that, “the quest for unity has become a specialty that narrows so intensely the intellectual focus of its devotees that they are unwilling to be interested in anything

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<sup>112</sup> House, *Establishing Priorities* (ref. 107), 2.

<sup>113</sup> Senate, *Superconducting Super Collider Project* (ref. 101), 13.

<sup>114</sup> In this context, the debate does mirror the conceptual issues Cat, “Physicists’ Debates on Unification” (ref. 7) identifies.



else in physics,” and asked, rhetorically: “Is that what we want to encourage when we speak of ‘the unity of physics’? Or does such ‘unity’ condemn one to a snobbish isolation from the mainstream of scientific and human concerns?”<sup>115</sup> Cranberg advocated accepting diversity as an equally potent ideal.<sup>116</sup> His criticisms of the particle physics agenda show that the same tensions that arose over high-stakes projects like the SSC were much deeper, shaping discussions of the discipline’s direction throughout the physics community.<sup>117</sup>

The same issue of *Physics Today* featured two articles from solid state physicists arguing for the importance of higher-level phenomena, shoring up the foundations of public arguments against the SSC, and favoring increased support for smaller projects. In the first, University of Chicago theorist Leo Kadanoff wrote on self-organization in physical systems, describing how plumes develop in heated fluids and finding “many different laws and many different levels of description.”<sup>118</sup> Kadanoff’s focus on complex phenomena complemented the perspective he had presented as a regular contributor to *Physics Today*’s editorial page since the mid-1980s. In 1986 he praised the ability to discern between practical and impractical demands for understanding and controlling complex systems as a key component of scientific judgment.<sup>119</sup> In 1988 he brought his notion of good scientific judgment to bear on contemporary trends in government science funding: “The true value of science is in the development of beautiful and powerful

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<sup>115</sup> Lawrence Cranberg, “The Paradoxical ‘Unities’ of Physics,” *PT* 44, no. 3 (1991): 102.

<sup>116</sup> *Ibid.*

<sup>117</sup> See: Cat, “Physicists’ Debates on Unification” (ref. 7).

<sup>118</sup> Leo P. Kadanoff, “Complex Structures from Simple Systems,” *PT* 44, no. 3 (1991): 9–11, on 10.

<sup>119</sup> Leo P. Kadanoff, “Cathedrals and Other Edifices,” *PT* 39, no. 11 (1986): 7–9, on 9.

ideas. Overinvestment in big science detracts from what is really worthwhile. I do not think that the nation's or the government's budget for research or for R&D is too small. It is, however, increasingly misdirected toward grandiose projects. We physicists have a responsibility to understand what is truly valuable in science and use this understanding to help the nation develop and express its priorities."<sup>120</sup> Close links between intellectual merit and broader relevance, of the type Anderson expressed before Congress and Kadanoff articulated in *Physics Today*, became critical the solid state community's outlook on the organization of science in America in the 1980s and 1990s.

The second relevant article in March 1991's *Physics Today* came from the American Physical Society president, Cornell's James Krumhansl. The piece was based on his outgoing presidential address at the 1990 APS meeting in Washington. The speech itself came only two months before Krumhansl wrote to journalist Malcolm Browne, who himself had published an editorial in the *New York Times* raising doubts, "as to whether the new knowledge it [the SSC] generates will be commensurate with the enormous cost."<sup>121</sup> Krumhansl's letter, which was entered into evidence during a 1992 Senate hearing, charged that: "extravagant representation to the public of the potential fruits from the SSC was fictitious and ethically irresponsible and that accurate acknowledgement was not given to researchers in many other subfields of physics which were the true source of contributions from physics to medicine, technology, economics and education but imputed to particle physics."<sup>122</sup>

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<sup>120</sup> Leo P. Kadanoff, "The Big, the Bad and the Beautiful," *PT* 41, no. 2 (1988): 9–11, on 11.

<sup>121</sup> Malcolm W. Brown, "Big Science; Is It Worth the Price? – A periodic look at the largest new research projects," *New York Times*, 29 May 1990.

<sup>122</sup> Senate, *Importance and Status* (ref. 96), 13.

Krumhansl's retirement address, and the article that grew from it, were more diplomatic than his private correspondence with Browne, but the message was the same: unity does not mean reduction, but rather the relevance of knowledge at all levels to the whole of physics.<sup>123</sup> His message complemented those of Cranberg and Kadanoff. Anderson himself joined the exchange a few months later, writing: "With the maturation of physics, a new and different set of paradigms began to develop that pointed the other way [from reductionism], toward developing complexity out of simplicity."<sup>124</sup> Diversity and complexity, for solid state researchers working in the shadow of particle physics, were critical prerequisites for unity. They indicated that physical knowledge was interdependent rather than hierarchical, which in turn implied a methodological rather than a theoretical unity, and cemented their claim to fundamental knowledge. As Cat has argued, the reductionist unity particle physicists sought was counterproductive for the goal of methodological unity.<sup>125</sup> This shared belief gave solid state researchers a singleness of purpose throughout the SSC debates.

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<sup>123</sup> An example of difference in tone: whereas Krumhansl's letter to Brown called SSC spin-off claims "fictitious and ethically irresponsible," the same complaint in *Physics Today* reads: "our need to obtain patronage seems to force us into extravagant claims and the hard sell. In my view, what is most dangerous in such statements is the certainty that we imply for the 'benefit' of a public unable to judge the validity of claims." James A. Krumhansl, "Unity in the Science of Physics," *PT* 44, no. 3 (1991): 33–38, on 38.

<sup>124</sup> Philip W. Anderson, "Is Complexity Physics? Is it Science? What Is it?," *PT* 44, no. 7 (1991): 9–11, on 9.

<sup>125</sup> Cat, "Physicists' Debates on Unification" (ref. 7).

By the 1990s solid state physicists were expressing a consistent view of fundamentality, at least for a national audience. It was similar to what Francis Bitter and Alvin Weinberg described decades earlier. Solid state physicists championed a permissive approach that promoted research across disciplinary boundaries and encouraged diverse applications. Without the reductionist hegemony and its influence on government science funding, though, the position championed by the solid state community would never have developed to the extent it did. Virulent reductionism, and the success of its proponents, forced solid state physicists to develop their views on why knowledge of complex systems was not less fundamental than knowledge of simple systems, and on how money and prestige should be distributed accordingly. Anderson's "More Is Different" responded to brewing discontent in the solid state community while physics was at the height of its prestige. Likewise, the resurgence of the Weinberg Criterion among solid state physicists, Anderson included, during the SSC debates was driven by angst over conditions that might allow the SSC to dominate the funding landscape as physics's prestige was waning.

In this particular skirmish, the permissive view of fundamentality prevailed. That is not to say that philosophy sank the SSC.<sup>126</sup> Rather, a complex set of shifting social and political factors brought the views of influential solid state physicists more in line with the goals of Congress. An evolutionary metaphor is appropriate: changing ecological conditions conferred a selective advantage to a particular conception of science. The change in conditions is evident from the

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<sup>126</sup> Kevles, "Death of the SSC"; Riordan, "Building the SSC"; and Hoddeson and Kolb, "Frontier Outpost" (ref. 90) each provide an account of some of the manifold other factors that led to the SSC's demise. See also: Michael Riordan, Lillian Hoddeson, and Adrienne Kolb, *Tunnel Visions: The Rise and Fall of the Superconducting Super Collider* (Chicago: University of Chicago Press, 2015).

success anti-reductionist rhetoric enjoyed before Congress. It is also clear from SSC advocates' efforts to show that particle physics was not isolated from the rest of the sciences, to claim that reductionist knowledge was prerequisite for true understanding of higher levels, and to promise that the SSC would produce a vast array of technological spin-offs. SSC advocates recognized that the Weinberg Criterion resonated with Congress. Their attempts to exploit it were not so well coordinated as those of solid state physicists; however, those attempts do provide further indication of how the political environment changed after the Cold War. Physics was no longer the darling of government patrons, but philosophical diversity within the physics population allowed the physics community to adapt to a changing ecological landscape and frame itself compatibly with the new context, even if such compatibility came at the expense of its pride of place within American science.

#### [FIRST LEVEL HEADING] CONCLUSIONS

The preceding case studies have shown how philosophical outlooks, as elements of scientific discourse, shaped the American physics community's internal politics and external interactions. These outlooks were, in turn, reshaped by this interaction. The conceptual details of scientific research alone did not bring about strongly articulated philosophical positions within the physics community. Instead, the institutional, financial, and political context in which that research took place catalyzed such views to grow from looser pre-existing convictions, which could also claim institutional roots. The historical study of these views, their origins, and their development can illuminate science's structural evolution through the second half of the twentieth century. This section synthesizes the conclusions drawn from the examples above and

offers a brief sketch of how the way I have used scientists' philosophical commitments here can be more generally applicable in the history of science.

Scientists' convictions about fundamentality operated at every level of scientific organization. Individuals with clear commitments sought to implement those commitments when they functioned as institution builders, as seen through the example of Francis Bitter and the National Magnet Laboratory. Changes such as growing funding asymmetries and the establishment of prestige hierarchies led individual scientists to craft and defend philosophical positions, as Anderson did with "More Is Different." The case of the SSC reveals how the compatibility or incompatibility of scientists' philosophies with national goals influenced the way the government supported scientific research. The story of how philosophical commitments were developed and deployed by solid state physicists mirrors the development of the field itself: from one of many new postwar subfields, to embattled physical sub-discipline, to mature specialty flexing a small measure of policy influence.

The National Magnet Laboratory exemplifies how individual scientists' commitments can guide the structure and mission of individual research installations. This was particularly true in the early post-World War II era when an abundance of government funding, with relatively few strings attached, meant that scientists enjoyed a large measure of autonomy when designing laboratories and assembling research groups. Internecine tensions ran low. The National Magnet Laboratory reflected Francis Bitter's desire to pursue fundamental questions and train students to ask them. His conception of fundamentality, which emphasized the formulation of widely applicable theoretical principles and power of research to motivate further research, produced a lab that promoted cross-disciplinary dialogue and a close interaction between theory and its applications. The NML embodied the view of fundamental research that helped organize

American solid state physics. An emphasis on what I have called fecundity and a willingness to draw insights from chemistry, metallurgy, engineering, as well as from other areas of physics, facilitated solid state physicists' efforts to unite a wide range of research programs within the institutional confines of a new sub-discipline. Bitter's brainchild was representative of a field that sought fundamental insight at all levels and applied it as broadly as possible.

Although a permissive approach functioned well within the laboratory, changes in the way the government funded science limited the power of NML administrators, and of solid state physicists generally, to fully determine the direction and scope of their research. Justifications that worked well for structuring a field's internal interactions were contingent upon their compatibility with larger social priorities. This dependence grew as physical research demanded greater levels of financial support. Budgets tightened and competition between solid state physics and a dominant particle physics community escalated through the 1960s and 1970s. The philosophical preference for viewing fundamentality as attainable by physical research at all levels, with which solid state physicists defined their early research programs, evolved into a justificatory mechanism aimed at external funders and competitors within the scientific community. Philip Anderson's "More Is Different" developed solid state physicists' folk understanding of how fundamental research should be carried out into a description of fundamental scientific knowledge. Whereas the conceptions of fundamentality offered by Francis Bitter and Alvin Weinberg were fully relational, and therefore non-hierarchical, Anderson's view conceived of fundamentality within a traditional hierarchical scheme that posed no challenge to the pride of place physics enjoyed in Cold War America. Anderson, and later Dresden, departed from previous views that saw fundamentality as a measure of utility for further work. In doing so, they staked out a more general position on what it meant to have

innately fundamental physical knowledge—knowledge composed of concepts that could not be attained by deriving them from something more basic.

Anderson was motivated to articulate his views by the fight for resources, but financial struggles were not his only reasons. Particle physicists, by conflating fundamentality with reduction, threatened to monopolize the considerable prestige physicists enjoyed in the Cold War era United States. Particle and solid state physicists developed philosophical viewpoints that reflected the exigencies of doing expensive research in a competitive funding environment with a substantial degree of social approbation at stake. For particle physicists, a restrictive account of fundamentality justified federal funding for expensive installations with little hope for generating practical social or technological results. For solid state researchers, a permissive account, sharpened in reaction to reductionism, underwrote their claims to an equal slice of the basic research funding pie and equal or greater relevance to national priorities, both social and technological. Solid state physicists' rapid response to the rise of reductionism complicates the picture that places particle physics at the nexus of philosophical and conceptual unity in the 1960s and 1970s.<sup>127</sup>

The sharpened perspectives that both solid state and particle physicists developed through the 1960s and 1970s provided the framework in which the SSC debates unfolded. Each view was deeply entrenched, to the point where the debate did not occur between physicists, but developed as each side reformulated old arguments for new audiences: Congress and the American public. The prevalence of these views, both in their expression on the floor of Congress, and their further dissemination through the popular press, meant that Congress members voting on the SSC were not faced with a choice between funding science and not funding science, but rather with a

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<sup>127</sup> As in Stevens, "Fundamental Physics" (ref. 8).



choice between competing perspectives on what science should be seeking. Both Philip Anderson and Steven Weinberg, representing solid state and particle physicists respectively, presented a broad picture of how science should be done, shifting the debate away from the details of the SSC and onto the larger question of how science could best serve society. Anderson and his colleagues capitalized on their opportunity to influence national science policy by offering a contrast to reductionism and providing congressional opponents of the SSC a positive view of science to support and a mechanism by which to deflect the anti-science stigma that might have come from an anti-SSC vote.

The SSC's story indicates the complexity of how philosophical outlooks operated at the highest levels of the national scientific infrastructure. Advocates of both restrictive and permissive approaches to fundamentality advanced competing ideals of scientific success from the mid-1960s through the 1990s. Their relative success on a national level was predicated upon shifting external conditions. The end of the Cold War, growing economic concerns in the early 1990s, and a freshman class of Representatives bent on sweeping spending cuts constituted a dramatic change in how members of Congress integrated science into their conception of the national interest.<sup>128</sup> Congress moved from a willingness to fund large, expensive projects for the sake of national prestige and the slim chance of a major technological breakthrough to a more circumspect approach to the national science budget, in which specific deliverables should exist to guarantee a material return on investment. This change threatened the privilege of reductionist physics, which had historically operated on a mandate built from intellectual merit and only the remote possibility of technological payoff. Hoddeson and Kolb, quoting particle experimentalist David Ritson, point out that as the Atomic Energy Commission began morphing into the

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<sup>128</sup> Kevles, "Death of the SSC" (ref. 90).

Department of Energy in 1975, physicists “became ‘just another group of constituents.’”<sup>129</sup> The clout physicists, and particle physicists in particular, once wielded through the federal advisory apparatus had evaporated by the early 1990s. A philosophical basis for scientific research that allowed fundamental knowledge to coexist with a more concrete promise of technological applicability and cross-specialty relevance better complemented the prevailing political ecology.

The role philosophical principles played in shaping the institutional missions, hierarchies of scientific communities, and the relationships between science and the broader society, changed at each level of interaction. Its importance on each of these levels indicates the extent to which philosophical considerations play a role in how scientists conceived of and ordered their professional activities. Scientists’ philosophical commitments are relevant not only to questions about how they conduct and interpret their research, but also to questions of how science is organized. Philosophical considerations are largely absent from institutional histories of later twentieth-century science. The cases considered here show, however, that they had a recognizable impact on the organization of American physics.<sup>130</sup>

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<sup>129</sup> Hoddeson and Kolb, “Frontier Outpost” (ref. 90), on 308.

<sup>130</sup> This discussion focuses on physicists, but they were not the only scientists with robust philosophical agendas in the later twentieth century. Much has been written on reduction in twentieth-century biology, for example, and although I have not attempted to draw connections with that story here, the national ascendance of biology is necessary to understand the reorientation of physicists’ expectations at the end of the century. For a recent survey of philosophical debates over reduction in biology, see C. Kenneth Waters, “Beyond Theoretical Reduction and Layer-Cake Antireduction: How DNA Retooled Genetics and Transformed Biological Practice,” in *The Oxford Handbook of Philosophy of Biology*, ed. Michael Ruse

Further investigation can reveal how philosophical views function similarly or differently at, and also between, various levels of scientific organization. I take the position, supported by the case studies above, that these questions must be answered within their specific domains. An analogy to Anderson's argument in "More Is Different" is fitting: it is an empirical question how philosophical positions affect change at different structural levels; we cannot divine the results until we examine the relevant behavior at the appropriate scales. The articulation of philosophical views by influential scientists does not guarantee their manifestation within the national structure of science. The process through which those views exert their influence, and change in response to shifting external conditions, is much richer. The historical impact and development of these views in professional and institutional context therefore represents a fruitful area of research for historians of science.

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(Oxford: Oxford University Press, 2008), 238–262. Kevles discussion the SSC in relation to the Human Genome Project, and argues that big science in biology was better adapted to the post-Cold War climate, in "Big Science Big Politics" (ref. 92).

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